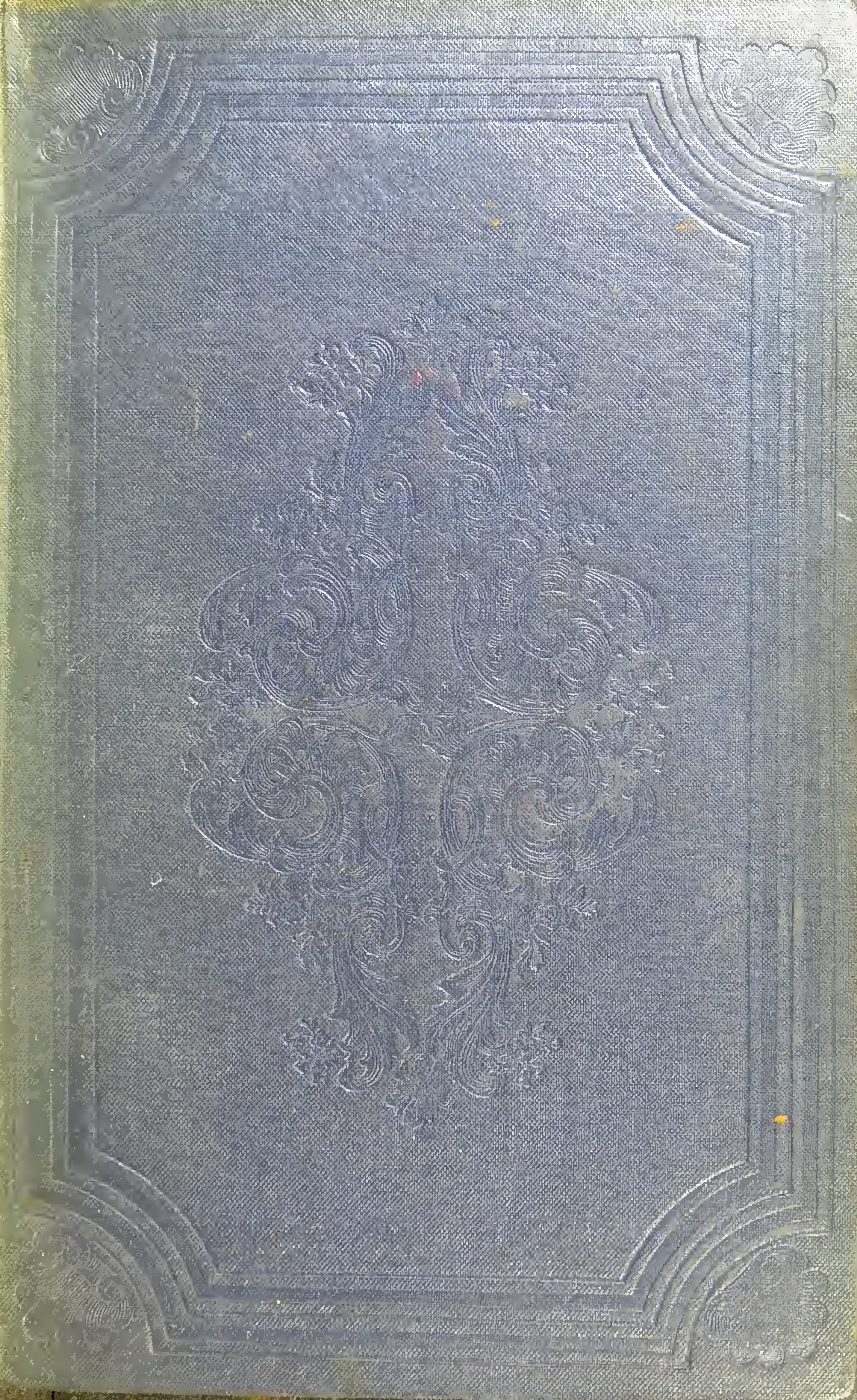

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OBSERVATIONS

ON

TUBERCULOUS CONSUMPTION;

CONTAINING NEW VIEWS ON

THE NATURE, PATHOLOGY AND CURE

OF THAT DISEASE,

BEING AN ATTEMPT TO FOUND ITS TREATMENT ON

RATIONAL PRINCIPLES,

DEDUCED FROM PHYSIOLOGY AND CONFIRMED BY EXTENSIVE APPLICATION.

ILLUSTRATED BY COLOURED DRAWINGS.

BY J. S. CAMPBELL, M.D.
Member of the Royal College of Physicians,
SENIOR PHYSICIAN TO THE ST. MARY-LE-BONE GENERAL DISPENSARY, ETC. ETC.

> " In order to make a direct attack on the disease (Phthisis), we ought probably to be able to correct an *unknown* alteration in the assimilation or nutrition, that is an alteration in the fluids of the body."
> LAENNEC, *Forbes's Translation*, p. 371.

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PREFACE.

When an author proposes to add another Treatise to numbers already published on the subject he undertakes to discuss, it behoves him to show that he has a rational prospect of either adding to the general stock of ideas on the matter of his discourse—or, of placing old ones under a new aspect—or, of improving their arrangement; unless one or other of these purposes be gained, it would be perhaps better, both for himself and the public, that his pages had never seen the light.

I will not dogmatically assert that this book fulfils any one of these intentions, for a writer is ever a bad judge of his own capacity—this must be determined by others: but I may with great confidence declare, that these sheets would never have been printed, had I not *believed* them to contain some new truths, and to develop views of considerable practical importance.

The whole phenomena of tubercular consumption seem to point out, that it ought to be considered as a compound disease. It is impossible to suppose that the adventitious matter which constitutes its immediate cause, is merely a growth, or product, springing out of the living textures which form its seat; and we are thus compelled to consider it as an effect of some aberration from healthy action, deeply rooted in the system. This indeed is an opinion, the truth of which is now very generally admitted, but seldom so strongly urged or well illustrated as in the writings of Sir James Clark and Dr. Tod.

But valuable as the labours of these authors are, they still leave a link wanting in the chain of causes and effects which connects early errors of digestion with final pulmonic deposition; for it can scarce be doubted that the entire symptoms of strumous dyspepsia may exist without being succeeded by pulmonary disease; and this fact is but vaguely explained when simply referred to the presence of a phthisical or strumous diathesis.

The link thus wanting I have endeavoured to supply, chiefly by examination of the capillary circulation connected with the right side of the heart; arriving at the conclusion, that while the properties of the blood in phthisis appear to be altered by a primary error of digestion, the immediate determining cause of tubercle is to be traced to certain vices of organization, inherent in the capillaries of the pulmonary artery; these bodies being, in fact, constituted of minute globules originally existing in the blood, and retained in those vessels during its transit through the lungs. These vices of vascular organization I presume to be hereditary, and so far as the individual is concerned, unalterable; and they constitute, as I believe, the essential element of what has been named a strumous constitution, which it requires only an abnormal condition of blood to call into full local development: a considerable share of this idea is hence based on the recognition of a humoral pathology—a term which not very many years ago it would have required some nerve to employ, except in disparagement; but which, in the revolutions of medicine, seems at length destined to play its legitimate part in the investigations of general pathology. To this phase of the disease I have ventured to apply the name of "formative" stage of phthisis, simply because it appeared to indicate, in brief terms, the idea I felt desirous to convey; and, in discussing its treatment, I have endeavoured to inculcate, that it is by changing the quality of the blood alone that we can ever hope to effect any useful end. The remedies most effectual in fulfilling this intention I have, after some considerable experience, concluded to be the caustic alkalis.

But the morbid phenomena of phthisis are clearly not all dependant on its formative stage. Organs, whose functions are so important, nay, so immediately necessary to the support of life, as are the lungs, must instantly manifest the presence of any local disease by effects produced on remote parts. This is in truth the case, and every organic or functional malady of the lung does induce effects, greater perhaps than the same amount of disease in any other organ produces. Of this general truth there can be no question; but, it appeared to me that in phthisis the morbid influence propagated to other organs from the central disease was, in many cases, greater in proportion to the amount of organic change, than when portions of the lungs were rendered functionarily useless as a result of inflammatory action. The reasons of this difference I have endeavoured to point out; and the conclusions arrived at are reported in the chapters which discuss the pathology and treatment of consumption, in what I have designated its stage of *reaction,* as contradistinguished from that of *formation.* On the management of this stage I have commented at some length, attempting to estimate at their proper value, the various discordant medicines, and methods of treatment which we find so profusely scattered over works directed to the subject, and thus to deduce certain principles which may, in some degree, guide the practitioner.

The intention of this book being to investigate the pathology and cure of phthisis as it actually exists, I have assumed its presence while dealing with the subject, and have consequently neither entered on the statistics of the disease, nor on a consideration of the remote causes which are most influential in calling it into existence. The vast importance of all such investigations no one will more willingly admit than myself; but they are already ably and amply discussed in other works, and my apology must be, that to encounter all points connected with so wide a subject was beyond my power. As the field is extensive, so ought the reapers to be numerous, and my highest ambition will be gratified, if I have been able to gather even *one* sheaf into the barn.

I ought, perhaps, to offer a similar apology on the subject of physical diagnosis. It is but slightly noticed in the progress of this work, and that chiefly at its termination, where it became necessary to show that the cases treated had been truly cases of consumption. It would be to me a subject of regret that any inference should from this be drawn, that I undervalue the advantages which have arisen from applying another sense to the investigation of disease; but we are bound to estimate this at its proper value, as a means only to an end. The most perfect diagnosis does not always point out the road to cure.

On the last word thus casually used I may be permitted to offer a few remarks. It is contained in the title page of this book, and some friends, on whose judgment I place considerable reliance, have viewed it as objectionable. Had I employed it as holding out the hope that all cases of phthisis are susceptible of removal, or, that lungs once beset with tubercles, can ever be so restored as to render them again healthy, I should have been guilty of a positive deception; because my own conviction is against the possibility of any such occurrence. But defining the word "cure," as I have done in the body of the work (p. 223), the case is altogether altered, and I believe that we are often as much gratified in employing the term in reference to phthisis as we are in other organic affections, where appropriate treatment, without absolutely removing the disease, renders it, with care on the patient's part, innocuous.

Of the numerous imperfections of this work I am fully aware. Should it attract any attention at all, it is more than probable that many of its pages afford legitimate subjects of criticism. The subject, however, it must be admitted, is one of high importance; and while I neither solicit favour or deprecate censure, I cannot help entertaining an innate conviction that its pages will add in some slight degree to a right comprehension of tuberculous consumption.

Weymouth Street,
7th October, 1841.

SOME OBSERVATIONS
ON THE
NATURE AND CURE
OF
TUBERCULAR CONSUMPTION.

Preliminary Remarks on the Connexion between the Blood and Life.

Every natural science proposes as its object, a reference of the varied phenomena which occur among the material constituents of the earth to certain general laws.

As we have no knowledge *à priori* of these laws, such acquaintance with them as we *do* possess has been exclusively obtained :—

First, by carefully investigating effects ; and,

Secondly, by inferring or deducing the law from the unvarying nature of such effects when occurring under similar circumstances.

The first man who observed that a stone or other body when removed from the earth tended to approach it again unless restrained by some counteracting power, or the first who observed that an acid united with an alkali, and thus formed a new substance which differed from either of its constituents, could only have regarded those phenomena as isolated facts ; but when these and others analogous, observed during a long series of years, and in an immense variety of examples, were found to be always similar under the same circumstances, men doubtless began to reflect on their cause, and the result of the investigation has been the reference of one and all of the changes and motions of inanimate matter to two principles alone—those namely of *gravitation* and *chemical attraction.*

But it is one thing to infer with correctness the existence of a principle, and ascertain in some degree the law by which it acts; and another to acquire a knowledge of such principle in its essential nature : and hence, though we perhaps possess a perfect knowledge of the law of gravitation, and a tolerably precise knowledge of the laws of chemical affinity, we are yet profoundly ignorant of the nature of those attributes or qualities of matter on which they depend.

But if this be true as regards *physical* phenomena, still more obviously true is it that we are profoundly ignorant of the principle on which *vital* phenomena are dependent.

The causes of this difference in the relative precision of our knowledge of physical and vital laws may, to a certain extent, admit an explanation.

The sequence of cause and effect is in physical phenomena uniform and unvarying under the same conditions; and these conditions being given, we can very often predict the results.

For example, in chemical science a long series of observations has shewn us, that one substance stands towards others in such relations that their particles tend to unite when brought under certain circumstances into close vicinity, but do not unite unless these circumstances exist; thus it often happens that two bodies whose particles strongly attract each other at a high temperature, or when moisture is present, are perfectly inert at a low temperature, or when moisture is absent, while in very many examples the conditions necessary to their union are even more numerous and complex.

The necessity of studying and duly appreciating the influence of these accessary conditions, considerably complicates the science of chemistry, and hence, although great advances have been made towards a precise acquaintance with its laws, they are still much less definitely known than the law of gravitation. This, precisely for the converse reason, is much more perfectly understood; and perhaps we are warranted in holding that, altogether independent of the fortunate application of a wondrous mind like Newton's to its development, there is inherent in the subject itself a greater probability that it should be fully developed, than that the laws of chemical action should be fully apprehended.

The condition necessary to the exercise of gravitation is one alone, namely, that the mass *attracted* should contain fewer particles of matter than the mass *attracting;* and provided this exists, the sequence of cause and effect is certain and unvarying.

Thus a mass of matter gravitates towards the earth precisely with the same certainty, and with a velocity regulated by one simple law, whether it be hard or soft, moist or dry, at an elevated or at a low temperature ; all of which variable conditions would more or less have modified the exertion of its chemical affinities, and thus presented us with a problem, whose difficulties are increased by the number and complexity of the elements required for its solution.

It would appear then that the difficulties of attaining a perfect knowledge of any natural law, are to a certain extent dependent on the number and complication of the accessary conditions by which the manifest phenomena resulting from such law, and from which its existence is inferred, are modified ; and it is probably very much for this reason that our acquaintance with the laws of vital phenomena is unmeasurably less perfect than that which we possess of those inducing physical phenomena.

A living body, presenting motions which are only explicable by a reference to vital force, exhibits besides, many others which are obviously referable to causes purely physical in their nature : we have thus presented to us a microcosm in whose motions or changes we recognise the operation of three distinct forces, vital, chemical, and mechanical; a circumstance which renders the study of the science of life considerably more difficult and complex than that of any other natural science whatever.

It is, for the reasons now advanced, impossible to define life, except by contradistinction : all we know with any certainty is this, that organized bodies under particular conditions present in each and every part certain motions or changes which are absolutely inexplicable on the laws of either chemistry or mechanics ; and to the *cause* producing such motions we assign the name of vital principle, or vital force—Life, as the word is usually employed, expressing the assemblage of manifest phenomena which result from its exercise.

But although thus ignorant of the cause of life, it is still desirable, for the better acquaintance with diseases, to endeavour at obtaining some knowledge of the laws by which it acts, for as diseases doubtless depend on certain aberrations from nature in the motions of the body, or on changes in its structure, so it follows that an intimate knowledge of them can never be acquired so long as our acquaintance with natural or healthy action is imperfect ; and it is doubtless from the want of such preliminary information that we are frequently compelled to minister to diseases in an empirical manner, and without any clear conception of the exact nature of that deviation from healthy action in which they consist.

For the attainment of this end no course appears better adapted than that of attempting to discover what motion or change is present under all the modifications of life as manifested by organic bodies; what *one* condition appears connected with vitality in the most extended sense of the term, which being present, life remains, but if taken away, life ceases.

As we cannot define *life* except, as we have already said, by contradistinction, the first step in the inquiry proposed consists in conveying some precise idea of what is meant by the term.

This we employ in two distinct acceptations. Employed in one sense, we use it to express the full performance of, or the existing capability of performing, the many motions which we know from observation and experience any special organism ought to be capable of exercising, according to the position it occupies in the extensive scale of living bodies. Here we employ the word to indicate the entire and complex life of an organized structure, a compound as it were of many vital motions which vary in number, in kind, and in degree.

But again we use the term life to express the actual exercise, or the existing capability of exercising any one or more of those motions which in their aggregate constitute a perfect life, others being either for the time dormant, or the capacity of performing them being destroyed.

Thus, when we speak of the life of an animal, we comprehend under this name, the acts of sensation, locomotion, intellectuality, nutrition and reproduction—these constituting as it were the *sum* of its existence; and when we speak of the life of a vegetable, we mean to include in the idea all those acts with the exception of the three first; but in each case the entire life is so constituted, that so long as any *one* of those acts is executed, the organism in the strictest sense of the term—*lives.* Its life is indeed less perfect than before—but still *it lives.* These considerations lead us to view the life of an organized body as equally compound with the organs by which it acts—as composed of numerous actions—as they are of numerous tissues, which in their conjunct exercise constitute a perfect life; but many of which may remain quiescent, or even be annihilated without any absolute destruction of vitality.

Admitting then this strictly compound character of life—but avoiding all hypothetical speculations as to how any act of this compound is performed—we may with safety inquire, Is there any one general condition inseparably connected with the execution of each and all—any common sustaining cause which being applied, they continue—being abstracted, they cease?

Such universal sustaining power of one and all the actions of life, in all its infinitely varied modifications, from the lowest plant to the highest animal, is found in the continued application to the intimate structure of the part performing them of some fluid, similar, or at least analagous to blood, which has been subjected to the influence of oxygen. We know this fact beyond all question—but we do not know how this fluid acts in producing the phenomena.

The varying construction of the organic apparatus, by means of which this oxygen is brought to act on the fluid; or the different means adapted for ensuring the distribution of this fluid afterwards to the organs, do not invalidate the truth of this general proposition; whether the oxygen acts through leaves, or spiracles, or gills, or lungs—whether the blood it endows with vital properties, be transmitted by a single or double heart, or by vessels only, independently of any central organ, the leading fact remains equally certain.

The functions by which this vitalized fluid is prepared and transmitted, are those of respiration and circulation. They are always intimately associated, and conduce to the common end of supplying each part with its proper excitant to action.

On this account these functions have been named "vital functions," by which term it is meant to express not that they are the only ones on which life is remotely dependant—but those on which it is immediately dependant. To use a mechanical illustration, they produce the steam without whose instant and continued application the whole machinery ceases to act.

In all the higher animals certainly, and probably as a general attribute of animal life, a third *vital* function, as it is usually termed, is executed by the brain and nerves. The claims of the nervous apparatus to this designation may be considered of *two* kinds—the one immediate, but only in some degree apparent—the other remote, less obvious, and yet, in truth, much more real.

The first or apparent claim is founded on the fact, that all the more manifest phenomena of animal life are strictly dependant on the integrity of a nervous system; and it is hence natural at the first glance to conclude, that in such life itself resides; yet during sleep, and still more in *coma* from accident or disease, many of the motions which manifestly depend for their excitation on the nerves are suspended, yet life undoubtedly still continues.

The less manifest but far more real claim to the designation of vital function by the nerves, consists in this—that they excite the respiratory muscles into action, and thus indirectly induce the physical conditions on which the reddening of the blood depends: but it is here to be remarked, that this power of influencing respiration, is itself in turn strictly dependant on the reception by the brain, spinal marrow, and nerves themselves, of red blood, remotely produced by their action, while conversely, artificial delatation of the thorax after all nervous action has ceased, still reddens the blood—still sustains the action of the heart—and still supports for a time very many functions of life—excepting always those which are in their display inseparably connected with nervous energy.

We conclude on these grounds that respiration and circulation are essentially and fundamentally the two vital functions, appertaining to every form and every description of organized bodies, and that in strictness, the claims of a nervous system (superadded to the others in animals alone) to be so considered, consists in its aiding those in effectuating the great purposes of preparing and applying arterial blood to acting organs.

In all the higher orders of animals the influence of oxygen is immediately exerted on their blood during its passage through the lungs ; and mediately communicated to their organs through the intervention of this fluid, which after traversing the left cavities of the heart, and large vessels in connexion with it, finally reaches the extreme or capillary branches of the arterial tree.

While in progress through the lungs the blood undergoes a certain change, which may be briefly denominated its *pulmonic* change—conversely the blood so altered sustains, during its progress through the vessels of the body generally, another change, which may be briefly expressed by that of *systemic* change.

These respective changes are effected only at particular points of the blood's transit. If examined while yet flowing in the larger trunks of the arteries, or in the smallest subdivisions which admit of one vessel being distinguished from another, the fluid is found to present the same physical characters which it did when first altered in the minute vessels of the lung.

When again this blood is examined in the smallest vein which is appreciable by the senses, we find that it has lost its arterial character, and acquired the character of venous; and we thus determine two points between which this change has occurred.

The converse of this is true as regards the circulation in the pulmonic vessels: there the most minute appreciable artery contains black or venous blood, while the most minute appreciable vein contains red or arterial blood; so that here also we identify two points between which a change has been wrought on this fluid, the counterpart of that induced on it in the systemic vessels.

In considering then the phenomena of circulation without regard to the vital or mechanical causes of propulsion, but simply in relation to its physiological importance, the broad fact is this— that a stream of black blood formed at the extremities of the systemic arteries flows along the whole line of systemic veins, through the two right cavities of the heart, to the extreme terminal branches of the pulmonary artery; and that a stream of red or arterial blood formed at this point flows along the pulmonary veins through the two left cavities of the heart, to the terminal branches of the aortic system.

As regards the causes which induce these changes in the blood, there remains little if any question, that the one from black to red is produced by the agency of oxygen. With equal certainty may it be affirmed that the converse change, or that from red to black, owns as its cause the execution of those various motions which in their aggregate we term vitality.

For reasons also already stated, it is evident that those changes take place in the most minute vessels of either circulation respectively; but as the vessels of the living body are, if the expression may be used, infinite in their subdivisions—so the precise point at which venous blood becomes arterial—or arterial blood venous—are questions to be determined more by *inference* than demonstration.

But a mere difference in physical characters of the two bloods does not necessarily imply any distinction in their vital influence, and we are hence compelled to observe the effects which result from each, either in continuing, impairing, or destroying those intestine changes and motions of parts which constitute life.

When indeed experience teaches us, that a cessation in the function of respiration very soon suspends, and if continued even for a short time destroys, life; and when we couple this with the admitted fact, that the only immediate and appreciable result of respiration consists in producing in the blood what we designate its arterial change; we might be led to infer with great certainty that red blood is immediately necessary to life, and arrive, on these grounds alone, at the same conclusion to which direct experiment conducts.

It is here however not necessary to trust to inferential reasoning alone; the experiments of Bichat, conducted with every possible precaution, varied in almost every possible manner, conceived by a genius, and prosecuted with an industry nearly unexampled, have conclusively established the one main and leading fact, on which all physiological reasoning must rest, that for the sustenance of every vital act, the fluid which circulates in the vessels requires to be subjected to the action of oxygen, and then transmitted by some mechanism to the most intimate fibre of each acting organ; and that when its action has taken place, one universal effect follows—the conversion namely of red blood into black.

We conclude on these data, that under the influence of oxygen in the respiratory apparatus, the blood either acquires a something which renders it an appropriate supporter of vitality in parts, or loses a something, which for the time obscures its vital properties, and renders it an inappropriate agent for that purpose—while conversely in the minute vessels of the system, it either gains that which obscures its vital character, or parts with a something which, being taken away, destroys its action as a vital agent; nor do we arrive at this conclusion with less of certainty, because confessedly ignorant of the inherent property by which each organ, or each tissue of an organ, effects distinct actions under the stimulus of a common cause.

If it be demanded what this "something" is, which by its addition or abstraction exerts such an influence over the phenomena of life, we admit our entire ignorance of its quality or essence. Our senses afford no information beyond what is founded on the varying colours of the blood, to lead us to a knowledge either of its absence or its presence; the most delicate tests which chemistry can apply fail to announce any other decided difference* between the two varieties of the vital fluid; and all we know with any certainty is, that the absorption of oxygen and the evolution of carbonic acid constitute the most marked physical changes which accompany, at least, if they do not produce† it.

As we make no attempt here to explain the nature of vital acts, but merely to connect the occurrence of those with the coincident occurrence of a special change in the blood, so we do not deem it necessary to discuss the question whether this fluid be itself alive, or whether it merely acts as the excitant of life in the solid parts through which it flows, and in which an inherent vitality or principle of life, as it has been named, resides.

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* I am well aware that many attempts have been made to determine the chemical difference in the constitution of arterial and venous blood, but they appear to me to have as yet led to no very satisfactory conclusions.

† I have here said that we may explain the differences between the two varieties of blood, considered as a vital agent, either on the supposition that as arterial blood it loses a life-giving something which makes it arterial, or acquires some deleterious property which it parts with in the lungs. On either explanation the fact remains unchanged, that the acquisition of oxygen is necessary to establish the vital character of blood, and is hence the remote *cause* of all action, and that the loss of the quality so communicated in the systemic vessels is invariably the *effect* of all action. For the mere sake of brevity in expression, I shall in the future pages speak of the arterial blood as parting with something in the capillaries of the aortic system, but by no means intend to assert confidently that it does so, far less do

If indeed we conceive of life as a subtile entity, which for a time pervades organic structures, leading during its presence to the motions of these, and by its negation inducing what is termed death; it is just as easy to conceive that this mobile agent is contained in and circulated by the blood, as that it is engendered in the brain and diffused by the nerves, or in fact propagated

I attempt to conjecture what this something may be. To the chemical changes connected with respiration, I have only incidentally alluded, my leading object being to deal with the blood's changes considered in reference to vitality alone. However desirable it would be to ascertain with perfect precision the exact place and mode of action between carbon and oxygen—which we know to be intimately connected with the respiratory change—it may be fairly questioned whether this would throw much light on the nature of vitality itself; how shall we connect the abstraction of a little carbon from venous blood, or the addition to it of a little oxygen, with the marked differences between it and arterial, when considered as vital agents?

One fact which seems to strengthen the idea that the red blood really carries from the lungs to the system a "something" which is the *cause* of life, is established by the ligature of a large artery. If the arterial current be arrested in its course by the application of two ligatures, the portion of blood between them speedily loses its redness and becomes black. Are we to suppose that the life-giving ingredient has evaporated by the coats of the vessel when prevented executing the duty it is destined to serve?

from any particular centre by any mode of distribution. On this view of the question, the blood might be considered as the only portion of the body in one sense really alive, inasmuch as it undoubtedly induces the phenomena of life in all parts. But if, as seems more in accordance with observation, we conceive of life as consisting in certain changes and motions, varying in kind and in degree, occurring so long as an aerated fluid is applied to the different tissues which perform them, then does it seem more rational not to confine the attribute of life, in the present state of knowledge, exclusively to the solids, or the fluids either, but to admit that both live, or in other words, evolve life by their reciprocal reaction; for although it be obvious that no part exhibits vital phenomena unless supplied with blood of a particular kind, yet it is equally obvious that a certain integrity of structure, as well as capability of action, must continue in these before this blood can effect its purpose, and hence we seem led to the conclusion, that a due and natural relation must be maintained between the blood, the vessels which convey it, and the solid parts through which it is conveyed, before by mutual action and reaction they can exhibit those phenomena which we term vital.

So far then we appear to arrive at the conclusion, that the "life" of an animal, as the term is usually employed, is a compound of various actions, coexisting when the *life* is *perfect*, but still quite susceptible of separation without death, the opposite of *life* being necessarily induced, and, further, that the one leading condition on which all of their actions, whether in their collective or individual display, depend, is the application of aerated blood to the organs performing them.

The actions which thus constitute a perfect life in their aggregate exertion constantly vary. So long as any one of them is manifest, the organism must be said to live; but, to use a familiar expression, it lives faster or slower in proportion, first to the number which for the time are in course of execution, and, secondly, to the intensity or force with which these are performed. The *minimum* amount of action exhibited by a healthy animal is found during sleep; between this and the *maximum* an infinite variety of gradations may exist, this *maximum* being perhaps found where inordinate muscular exertion is co-existent with a highly excited condition of the passions or intellectual powers.

As we have previously assumed that arterial blood is the universal exciting cause of all action, it follows that there must exist a strict relation between the amount of action performed and of arterial blood consumed, or what amounts to the same thing, changed; and as actions ever vary both in number and in force, so it likewise follows that the aggregate quantity of red blood expended will vary in a like degree under particular circumstances.

Again, as the consumption of red blood is an effect common to all actions, so the very energetic performance of any one, may consume, under particular circumstances, as large or even a larger quantity of this fluid than numerous actions moderately performed.

Thus supposing the aggregate amount of action at any one time, throughout the body at large, or in a part only, to be represented by the number 10, the amount of arterial blood requisite for its support will be the same, whether this entire sum be constituted by 5 distinct actions, each represented by the number 2 . . . . = 10.

Or by 5 actions executed with varying degrees of force, among which 4 may be represented by

$$\left.\begin{array}{l}\text{the number } 1 = 4 \\ \text{and one by } 6 = 6\end{array}\right\} \cdots\cdots\cdots\cdots\cdots = 10.$$

This law holds good in all the varying states of living action, the total amount of arterial blood expended in the systemic vessels being strictly as the sum of action in the body generally, and the total sum consumed by a part being directly as the intensity with which the actions of that part are executed.

We base this not only on the admitted fact that the velocity of the circulating and respiratory functions, and, as a consequence, the quantity of red blood transmitted in a stated time through the body, is directly as the aggregate energy of its actions, but also that this blood is always changed in the capillaries of acting organs, and when we connect this with the fact that red blood alone sustains action, we appear warranted in concluding that the final purpose served by the augmented afflux of this fluid to a part when its action is high, and the diminished quantity when its action is low, is that of supplying it with variable quantities not of blood alone, but of the quality or matter, call it by whatever name we may, which when united with the blood renders it arterial.

But if it be true that the consumption of arterial blood is always in the direct ratio of action, an obvious necessity arises for the production of that fluid in such quantity as the existing velocity of such action may demand; and we are hence led to consider the question how this due balance is maintained between the organs which produce, and the organs which consume it.

Now the first observation which here presents itself is this, that the organs which produce and distribute arterial blood also consume it, and that the former function is only served by them so long as the second function is performed by their capillary vessels.

The thoracic muscles which dilate the thorax, and thus effect that physical change on which the primary step in the reddening of the blood depends, and the muscular fibres of the heart, and possibly of the larger vessels by whose contraction this fluid when formed is at least partly transmitted to other parts, are as strictly dependent on the contact of arterial blood for their contractions, their sensations, secretions, and sustenance, as are any of the other organs supplied with it through their means: they are hence self-sustaining organs, supplying to themselves the vital stimulus which they themselves produce and circulate.

The brain and nervous system, on account perhaps of their presiding over by far the most obvious and marked phenomena of life, appear to be more immediately connected with its actions than the allied (vital) functions of respiration and circulation.

It has hence been a favourite doctrine with many speculative physiologists to consider the brain and nerves as the generators and distributors of what is termed vitality. In this view life is considered as a subtle essence of some kind, which under the various names of *animal spirits, principle* of *life,* and numerous others, is presumed to be formed or secreted in the brain, and poured forth from the centre reservoir in streams of various magnitude, and with various degrees of velocity, as the exigencies of the body rendered necessary.

Thinkers of this class appear to have overlooked or taken very slight account of the fact, that life exists in a condition quite as perfect, though not so complex, in structures where no organs analogous to a brain or nerves exist, and by whom no functions are performed at all similar to those which, in the higher orders of beings, are obviously dependent on a nervous system.

They appear, farther, to have omitted a consideration of the fact, that even in organisms the more manifest of whose vital phenomena are clearly connected with the integrity of a nervous function, these may one and all cease, and yet life, in the stricter sense of the term, be continued. Considered indeed as a *perfect* life it has, under such circumstances, sustained a diminution by the suspension of many actions which nerves only can excite, but with regard to others, life proceeds—only in a less complex form than before.

The first of these propositions is proved by the whole vegetable kingdom : the vegetable displays of its kind a life as perfect, though not so complex, as the animal; it lives without nerves, but it dies unless supplied by its vessels with aerated fluid.

The second proposition is demonstrated to some extent in sleep, still more clearly by the effect produced on a part by the ligature or section of its nerves, and more markedly still in apoplexy or paralysis. In these last cases all the more manifest phenomena of life cease in the whole or in a part; there may remain no sensation and no motion, yet the body or the part dies not, it still lives, though more feebly than before.

In so far then as many of the more marked actions of life are concerned, it appears that they may cease in a part, or even in the whole body, under the deprivation of nervous energy, and yet that part or body still strictly lives. Nutrition and secretion, may still continue perfect; but if we reverse the case and ask do they equally continue to live when perfectly supplied with nervous energy but deprived of arterial blood? the answer must be in direct negation of the proposition.

But seeing that the primary step in the production of this stimulus depends on the contraction of the thoracic muscles, and that they apparently contract under the direct influence of nerves, we are naturally led to consider in what immediate relation the nervous system stands in regard to the actions fundamentally essential to the sustenance of life; for though it may be true that blood is necessary to such actions, it may be equally true that a nervous system is still more influential in their production.

If a part cannot act in any manner independently of the transmission of red blood to its extreme capillaries, and yet can partly act independent of the reception of nervous energy by its extreme nerves, in what manner are we to explain this? both conditions are obviously necessary to the existence of a full and perfect vitality, and yet life in some measure continues when one of these influences is abstracted.

In attempting the solution of this question it is necessary carefully to distinguish between the condition essential to the *execution* of a vital act, and that which *excites* or determines its execution.

To induce the contraction of a muscle, for example, two conditions are clearly demanded. 1st. It is necessary that an exciting cause should be applied, and it matters not for the argument whether this be the will or an external irritant, as in the voluntary muscles, or some peculiar and appropriate stimulus as in the case of those which are named involuntary; but in either case, one and all of these excitants fail to produce their effect, unless, secondly, there co-exists with the application of this cause, a free and continuous supply of red blood received by the artery of the part. Here the determining cause of contraction is one or other of the stimuli named, but the essential execution depends on the contact of red blood.

The same view we conceive applies with equal strictness, to a nerve of general or special sensation. Here, too, the nerve may be perfect in itself, and freely communicate with the central source of nervous power, and yet it performs no act of sensation independent of its artery: in such case the nerve seems placed in the same circumstances with the muscle; it determines, it is true, the occurrence of the sensitive act at its extremity, and it conveys to the sentient root in the brain notice of its occurrence, but it executes the act, or (if we may so speak) evolves sensation only so long as red blood continues to be supplied.

The same relation seems also to exist between the circulation and the brain itself. Although we are yet imperfectly acquainted with the precise functions of the various parts of this organ, yet all its phenomena warrant the inference that these are of two kinds; first, the regulation of the organic machinery; and, second, the performance of intellectual acts.

The brain, therefore, in the first case, evolves or secretes, or forms (use what expression we may) a certain something on which the nerves communicating with other parts depend for their functions—a something which must continue to pervade the nerve from its source to its extremity before any of the actions it presides over are accomplished; and, in the second place, the brain, independent of forming this nervous power, on which the action of other parts depends, also, as an organ, forms, or secretes, or evolves (call it again by whatever name we please) certain ideas or thoughts which, in their combined execution, constitute the intellectual actions of the body, using this term in its most extended sense.

These functions we are bound to presume the brain performs in some way or other, as a consequence of its own peculiar structure, just as a muscle contracts, or a nerve feels, or a gland secretes its special fluid; the action is peculiar to itself, and must be viewed as a final cause; but in the case of the brain, just as in others named, the organic integrity may remain perfect, the inherent capacity of action may remain entire, and yet without the application of the one special condition no result follows. Arterial blood must permeate its vessels, or all action is suspended.*

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* In thus assuming the brain or rather a portion of that organ to be the material seat of the intellectual powers—and in still further assuming that it performs those functions or *evolves ideas,* under the stimulus of a cause common to all vital action, I rest on physiological facts, which admit of no dispute. By

We appear then fully entitled to conclude, that *all* vital acts owe their execution to one common cause, the reception of arterial blood, by the organs in which they occur, and are followed by one common result, its conversion into venous. Further, we are justified in inferring that this law is not invalidated in the case of sensation, either general or special, or in that of the action of the brain, but that each sensitive and mental act is just as strictly the result of arterial action impressing particular organs, as the contraction of a muscle, or the secretion of a gland, is the result of its action on the tissues by which those functions are performed.

It follows that actions *excited* by, and in themselves dependent on, the brain and nerves, are really *executed* under the contact of arterial blood, and followed by its change. A question there-

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many persons however, whose reflection has not perhaps been quite equal to their good intentions, such idea has occasionally been held as leading to a wretched materialism altogether subversive of religion.

I here then, in the most express terms, repudiate an opinion which might be attached to me by an erroneous interpretation of the text. That it has pleased the great God so to connect in this life the spiritual and material parts of man, that all the manifestations of the one are strictly dependant on the structural integrity and healthy action of the other, I cannot for a moment question, on the evidence afforded by that intellect which He himself vouchsafed to confer: that when it shall please Him to dissolve the union between soul and matter, the former, different in essence and in destiny, will continue to exist for ever, I learn from the announcements of Revelation. The *little* we know of the actions of life has been gained by observation and experiment; the *all* we know of the nature and destiny of the soul, we learn from the gracious pleasure of the Being who created it.

fore occurs, whether the converse of this is ever true, whether under any circumstances arterial blood is ever converted into venous, entirely independent of nervous influence?

When we examine a paralysed limb this would seem to be the case, an arterial current still flows in its artery, and a venous current is returned through its veins. The blood has been changed in the extreme vessels, and yet in some instances the limb exhibits none of the phenomena which result from nervous action. Here, then, we appear to be furnished with an example where, by some power inherent in the textures or vessels of the organ, life is sustained, and arterial blood altered altogether independent of the nerves.

Such a conclusion, however, is perhaps premature in the present state of physiology.

Although we know in such cases that the nerves ministering to motion and sensation have lost their power, and that this is compatible with the continuance of life in the part, and consequently with the sanguineous change which always attends life, we are not equally prepared to say that the nerves presiding over nutrition and secretion are likewise impeded in their action, and it may be that the vital acts which are still manifested, and the change of the blood which still proceeds, result from a mutual action between the organic nerves and arteries of the part.

On the whole, then, it cannot with any certainty be concluded that the change of blood in an acting part ever occurs altogether independent of nervous power, but there can remain no question that when any action is performed, which is clearly excited by the nerves of animal life, the joint aid of red blood is necessary to its execution; whence it follows that such nerves when excited into action consume arterial blood in quantities proportioned to the intensity of their acts, and consequently influence the heart and respiratory organs in a manner not less decided than other actions, whose influence on the blood's motion is perhaps more unequivocally shown.

When the organs of the special senses are unemployed the arterial circulation in their capillary vessels remains at the lowest point. No more blood arrives at the part than is simply required for the purpose of nutrition, or at times secretion. When again the function is called into exercise, its artery instantly acts with greater force, its capillaries are more fully charged, and this increases in a direct ratio with the augmentation of its action, while at the same time an equivalent increase takes place in the current of black blood returning by its veins. How otherwise shall we explain this without admitting that the act of sense, and the changes of circulation connected with it, originate in the organ itself? the primary impression indeed being made on a nerve of peculiar function, but the act by which that nerve evolves the sensation being immediately dependent on the contact of its arterial current, and this too required in quantity proportioned to the *quantity of sensation,* if the expression be admissible, which is produced.*

Precisely similar phenomena, and explicable only on a similar principle, attend the unwonted exercise of general sensation.

Even while this increase passes not the limits of indifference, an unusual action of the capillaries attends it; as it exceeds this and merges into pain, the activity of the circulation still further increases; and when the pain is great, not only are the local signs of increase very often marked, but an appreciable and not unfrequently powerful influence is exerted on the general producing and distributing functions of arterial blood, respiration, and circulation.

Seeing then that in both the examples of action now recited, the act performed by the organs of special and general sensibility is simply the evolution of what is named sensation, in varying degrees of intensity; and seeing further that coincident with the greater or less amount of this evolution, a greater or less amount of arterial blood is changed in the organ, we cannot hesitate to admit that the contact of this fluid with the extremity of the nerve is the cause on which it is dependent for the execution of the act.

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* In dealing with this question, one is unavoidably compelled to talk of sensation as a "something" which, being created in greater or lesser quantity, influences the consumption of arterial blood. I must be considered, however, to use such a mode of expression for the purpose of illustration only,—as intended simply to show the close and instant connexion between a material fluid and the "sensation" its contact evolves; if we talk of the "number," "quantity," or "intensity" of ideas or thoughts, it seems equally admissible to apply such mode of expression to "sensations."

But if it be admitted that *special* or *general* sensation consumes arterial blood simply by the action taking place at the extremity where sensation is evolved, there appear to exist reasons why this should directly lead also to an increased consumption by the central portions of the nervous system.

If a nerve at its extremity depends on the circulation for effectuating its action, it not less depends on the reception of a peculiar influence from some central point for the excitation of such action. Again, this central part, be it the brain, the spinal marrow, or possibly the ganglionic roots, only produces this while supplied with arterial blood; whence it results that as each act of a nerve at its extremity necessarily leads to a corresponding action at its root—or rather to a corresponding action of such central part of the nervous system with which its root is connected, so an amount of red blood is consumed by the brain, &c., corresponding with the quantity consumed by the acting extremity of the nerve. In other words, whenever a nerve of general or special sense leads by its action to the consumption of red blood, we may fairly infer that the *aggregate* amount expended by the act will be compounded of the quantities directly and indirectly consumed. First, by the acting extremity of the nerve itself; and, secondly, by such portions of the brain or spinal cord as produce the influence or fluid which enables it to excite the action.

This argument, so far as it relates to the point immediately involved, might be carried even further than this.

It is probable that in all, and certainly in many cases where acts of either general or special sensation are performed, that they consecutively induce mental acts, or, in plain language, engender "ideas" in the mind. But the brain, at least a portion of the brain, is demonstratively in some mysterious manner the material organ of the mind, and, as such, only acts while supplied with red blood in quantity proportioned to the mental acts performed. Hence the execution of every action at the extremity of sensitive nerves becomes the remote cause of exciting certain special mental acts in the brain; and, consequently, the cause of augmenting the consumption of red blood by that organ.

Every act of sensation then appears to lead to the consumption of arterial blood, and, as a consequence, originates a necessity for its production in three ways:—

1st. *Immediately,* at the seat of action where the sensation is evolved.

2d. *Indirectly,* at the central point of the brain or spine where the influence pervading the nerve is produced.

3d. *Consecutively,* by inducing mental action as a sequel of sensitive action.

But if the actions performed by the central or ramified portions of the nervous system are thus demonstrably performed only under the contact of red blood, not less true is it that the same law regulates those over which the nerves by no means exert so manifest a control.

When nutrition is rapidly performed in a part, its arteries pulsate with unwonted force and speed; when it languishes, they become feeble in their beats and small in their calibre: in such cases the quantity of red blood which arrives at the part corresponds with the relative activity of the nutritive process, and a corresponding quantity of black blood leaves it. In other words, there is an abstraction or retention in the organ of a portion of *that* which constitutes the difference between the two bloods, corresponding with the activity of the function.

The same law applies to secretion. Many glands secrete only at particular times. In them the arterial circulation is nearly dormant when their functions are quiescent; when in action the flow of red blood towards them, and of black blood from them, is enormously increased. Similar phenomena are perceived in glands whose action is more constant. In all such the arterial current is weak or strong, quick or slow, in a direct ratio with their activity.

The voluntary muscles in contracting exemplify the same fact.

When a muscle is cut off from a supply of red blood it ceases altogether to act, no matter how powerful the stimulus may be ; when it acts only feebly, little red blood flows to it, when with energy its artery beats forcibly and more blood in a given time pervades it ; when its contractions are inordinately powerful the acme of its arterial velocity is acquired.

In all such cases, it need scarcely be added, the quantity of venous blood returned from the acting muscle corresponds to the amount of arterial blood received by it. This *fact* we find set down in all works on physiology, although it is explained in our conception on an erroneous hypothesis.*

Nor is the fact different as regards the involuntary muscles ; those of the *heart* itself, the great regulator of the circulation, receive by the coronary arteries, and consume in their capillary vessels, an amount of red blood directly as the energy of their action. The muscular coats of the stomach, intestines, bladder, &c., exemplify the same law.

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* I am far from denying that the contraction of muscles, as usually stated, tends mechanically to fill the veins by forcing blood along those tubes. I only mean to say that this will not account for the augmented quantity of black blood produced by the act, a quantity which augments as contraction increases, and diminishes as it declines. I know not how this can be explained except on the supposition that the contraction of a muscle exhausts red blood in quantities determined by its force. If we admit the additional flow of venous blood *from* a contracting muscle, we must also admit the previous afflux *to* it of additional arterial, and, consequently, admit also that an augmented quantity of the one has been converted into the other.

I am well aware that this exposition of the constant connexion between the *energy* of *action* and the velocities of the circulation, may be considered as stating only a fact already well known to every tyro in physiology. The fact is without doubt universally acknowledged, but it appears equally true that the inferences capable of being deduced from it, not indeed as explanatory of the essential nature of vital action, but of the effects produced by action on the circulating function, have been considerably neglected.

We find it, for example, constantly taught that a part acts strongly, because the heart has transmitted to it more blood than usual. We hear of stimulants augmenting the force of circulation by a *direct* action on the heart. We hear the hurried pulse and panting laborious respiration consequent on extreme muscular exertion explained on the principle that muscular contraction mechanically accelerates the flow of blood in the veins, and this without a single allusion to the important fact that the blood avowedly sent back in a venous state must have been supplied by a current of arterial blood at least equally large, and that during this change from red to black it has parted with the very quality which renders blood an appropriate supporter of vitality, and to convey which into the acting muscles must certainly have been the final purpose of its augmented flow. Those explanations I conceive to proceed on a hypothetical assumption altogether erroneous, in which cause is mistaken for effect.

When a living animal rouses from a state of repose or comparative quiescence into action, the contractions of his heart remain for a little undisturbed; as his action increases they by degrees increase also in quickness and in force. If this change be closely watched the increase in the heart's action will invariably be found to succeed the quickened systemic actions which it accompanies. The organs appear to originate, from an unknown but innate power; the motions in which each consists, and in so far as the sequence of phenomena can be noted, the heart *first;* and next, the respirations respond to this by an acceleration of the functions they respectively perform: the final purpose served being evidently the production and transmission of an increased quantity of red blood; and the remote exciting cause of such acceleration being the augmented consumption of red blood, and increased production of black blood by the organs in action.

Our conception therefore of vitality is briefly this—that it consists in motions originating in the intimate contexture of organs—that those motions increase or diminish therefrom various causes acting on the body in a manner quite inexplicable : the only point which we know, with any certainty, being this—that arterial blood acts as the sustaining cause of action, and that venous blood is produced as its effect.

But compelled as we here are to commence with the assumption of a *final cause,* it by no means follows that this should render us incapable of tracing the reciprocal action and reaction between the various organs of the living body, by which the continuance of this vitality is insured, and its varying conditions regulated. It is this which constitutes the legitimate sphere of physiology ; when we endeavour to grasp the essential nature of living action, clouds and thick darkness immediately beset our path : when we limit our inquiries to the mode in which it is supported by the reciprocal influence of different organs, and merely trace the chain of secondary causes and effects by which the living body exhibits itself as a *self-sustained machine,* we adopt perhaps the only mode of investigation which can conduct us to results really useful and important.

Any general view of physiology, conducted on such principles, is quite unsuited to our present object. I have introduced this section simply to illustrate certain subsequent remarks on a particular disease of the lungs, and must content myself with offering a few remarks on the mutual action and reaction between the vessels which sustain life on the one hand, and the organs by means of which those vessels are rendered capable of effecting such purpose—the heart namely and the lungs.

We have assumed then two general positions—

First, That vital action of any description, though its essential nature is unknown, originates in the intimate contexture of organs, and is immediately associated with their capillary circulation.

Secondly, That the actions so executed inevitably depend on arterial blood, as a cause, and produce venous blood, as a result.

Considering physiology then as properly dealing only with secondary causes and effects, the questions we have here to propose, are—

First, On what vital mechanism does it depend that the organs executing actions, which vary from minute to minute in number and velocity, always, in the natural state, receive the precise but variable quantity of red blood required ?

Secondly, How does it occur that within certain limits this quantity is invariably produced ?

The first of these purposes—that is, the reception of red blood by parts, in quantities varying as their actions vary, may, it is obvious, be dependent on one of two causes—either we may conceive that the heart, in some manner quite unknown to us, is directly influenced by an existing necessity for blood in a part, and that the additional flow towards it may be the effect of an additional exertion of the central organ—or we may conceive that as the necessity for additional blood, contingent on increased action, originates in parts themselves, so there may exist in them some vital mechanism in consequence of which they draw towards them by an inherent power—and in fact, as a consequence of their action—the amount of vital stimulus required for its support.

If we form our judgment on this question by a reference to the language usually held in physiological discussions, we must certainly conclude that the *first* of these opinions is the one usually adopted. The expressions " central organ of the circulation," " direct influence on the heart," " increase of the heart's action," as applied to explain all varieties of vascular excitement, obviously indicate that every increase in the quantity of blood which flows into an organ is attributed to augmented cardiac energy; that increased action of the heart is the *cause* of increased action in other parts.

Such an explanation is in the outset liable to this very serious objection, that it leaves entirely unexplained how an organ situated frequently at a considerable distance from the part in which increased action occurs, should immediately *perceive* the wants of that part, and as a consequence supply them.

But waiving this preliminary difficulty and assuming that by some means the heart may perceive as it were the wants of organs, we then encounter the additional one of comprehending how the heart, whose influence is *generally* exerted on all the arterial tubes connected with it, should under circumstances of increased action in *special parts* supply an augmented current to the arteries of those alone.

If I apply a mechanical or chemical stimulant to a part distant from the heart, I immediately find that its vessels receive an unusually large supply of blood; if the stimulating cause has acted only slightly, I shall probably be unable to appreciate any increase either in the frequency or augmented calibre of the artery which leads to it. I am only aware of the increase of its circulation by a visible change in the smaller vessels. If again the action set up in it be a little more violent, I not only perceive by this that more blood than usual reaches it, but I recognise a distinct increase in the volume of the arterial trunk supplying them, and this perhaps without any increase in the number of its beats. But when the local action reaches a certain point of intensity, not only do all these phenomena display themselves in still greater force, but I find also that the pulsations of the artery augment in number as well as in volume; and knowing that the numerical pulsations of an artery are always contingent on the numerical pulsations of the heart, I infer with perfect certainty, and even without examination of this latter organ, that an increase of action which seemingly originates in a remote part of the body, has, when it reaches a certain point of intensity, exerted an appreciable influence on the central organ of the circulation, or at all events, that the two phenomena coincide with each other.

We are usually in the habit of saying, when these circumstances arise in diseased states, that symptomatic fever is established, which very well expresses a fact of constant occurrence, but leaves its explanation unattempted.

Precisely the same phenomena which are in this illustration connected with a state approaching to diseased action, occur likewise as the result of increments of natural actions.

If I powerfully exert the muscles of an extremity, if I subject the brain to high and continued mental effort, if I stimulate the stomach by an excess of injesta, if, in short, I by any means exaggerate the vitality of any organ, or set of organs, one unvarying result occurs—an increase in the quantities of blood which these receive; nor is the fact less undoubted because, when the exaggeration is but small, it is announced only by the turgescence of the small vessels.

In this chain of causes and effects, increased local action forms the first link; increased arterial action, appreciable by the senses, the second; and assuming the phenomena to be correctly stated, we conceive that they are in no way explicable on the supposition that action, in so far at least as the amount of blood demanded for its support goes, is dependent on the *primary* adaptation of the contractions of the heart to the exigencies of distinct organs.

If indeed under all circumstances of increased action, such for example as occurs in general fever, we found each artery, even in its most minute subdivisions, acting with equal, though unusual force, we might possibly conceive that all were supplied with blood by the sole agency of a common cause; but when we see the arterial trunk which leads to one single organ only, unusually excited, when we see that a relatively larger quantity of blood flows into this than into other organs at the same moment—then does it appear that we cannot attribute this occurrence simply to the propulsive powers of the heart, but must refer it to an augmentation of vital or mechanical force, either in the small capillaries of the part, or in the larger vessels which supply them.

But independent of this argument, which is more or less drawn from the mechanism of the circulation, there is another, deducible from our acquaintance with the physiology of the blood, which appears to me of still greater force.

Observation and experiment equally attest that the purposes served by the circulation of the blood are directly connected with the change it undergoes during its pulmonary transit. The organ which acts strongly demands blood it is true, but it demands more—it demands arterial blood, and unless the fluid conveyed to it has sustained this preliminary change, its access would be useless. But in a case where only one organ out of many is acting with increased force, and when consequently we assume that arterial blood in additional quantity is required for the support of such action, it is obvious that a less relative quantity is required for such others as act with less force; and hence were all equally supplied with red blood, there would exist a waste of that fluid, and an obvious useless expenditure of the pulmonic action which produces it, quite incompatible with the economy of power which pervades the whole system of the animal body.

We here, however, approach the confines of a discussion quite unsuited to the present object, and on which we cannot enter. To indicate the precise amount of motive influence exerted on the circulation by the many forces which have been presumed to effect it, and which perhaps are all concurrent causes leading to a common end, would far exceed our present limits. Each physiologist in turn who has treated of the subject, probably exaggerates the importance of that physical or vital power which for the time it has been his object to uphold, and if the great Harvey was wrong in attributing too great an influence to the heart, not less wrong have those been who since his day have ascribed far too large a portion to one or other of the various collateral causes which, considering the vehemence with which their respective claims to notice have been urged, might be supposed to reduce the function of the heart to something like a nullity.

Avoiding, however, in a great degree, all discussion on the mechanical or vito-mechanical causes of the blood's motion, we may narrow this question by considering it in its physiological relations, and from these the deduction seems to us unavoidable, that by whatever means the mass of fluid is propelled, the causes of its propulsion, with varying degrees of velocity and force, originate in the capillary vessels of organs.

Let us ask the question, What is the leading purpose of the circulation? Surely we must reply, the application of red blood to the minute vessels of the body; for unquestionably by this only can its actions be continued. But the red blood thus transmitted is certainly not expended in the organs, a part only is lost, and the remaining portion is still capable of again becoming an appropriate stimulus to action as it was before. Certain mechanical arrangements (of which the moving power is vital) become therefore necessary, by which blood of one description may be transmitted to the organs, and blood of another kind received from the organs to be again placed under conditions which favour the re-acquisition of the quality it had lost in them.

The purposes served by the circulation consequently demand,

First, the existence of organs of transmission.

Secondly, of organs of reception.

Thirdly, of organs in which the blood received may be rendered fit for re-transmission.

In man and the higher animals these purposes are admirably attained by their possession, first, of a double heart; and, secondly, of lungs; the one heart receiving the exhausted or devitalized blood from the organs and transmitting it to the lungs to be revivified, the other receiving this vitalized fluid from the lungs and transmitting it to the organs to be consumed.

Now, in this important circle of causes and effects, the great difficulty is, to fix the point at which the actions commence; we see the left heart sending red blood to the organs—we see the organs expending red blood and sending back black blood to the right heart. Again, we see the right heart transmitting black blood to the lungs, and the left receiving red blood from the lungs, destined again to be transmitted to the organs; and in this complex series of actions, we desire to know at what point the primary impulse originates?

Does the left heart send arterial blood to organs *because* it possesses an intuitive perception of their wants?—or does the right heart send venous blood to the lungs *because* by this only can it supply the left heart with the particular fluid it requires?—or do both obey a common impulse communicated by other causes, being thus as it were the *servants,* not *masters* of the body—*obeying* not *dictating* to its organs, but transmitting to them the quantity of red blood which they solicit; and to the lungs, the quantity of black blood which they return?

In judging on this point, we have the high authority of Mr. Hunter, for saying that the original *necessity* producing action commences in the veins,[*] the heart consecutively contracting as this necessity is small or great; and the respirations, as we shall yet see, following in a corresponding manner the impulse of the heart.

As we possess no means of watching the actual commencement of action in living animals, our only mode of investigating the progressive motions of organs induced by it, consists in observing the phenomena attendant on its increments. This we can in some degree effect by observing an animal at the *minimum* amount of action compatible with healthy life, and tracing the changes developed as its actions become increased.

This minimum is doubtless found in sleep, which constitutes therefore the most favourable point at which the inquiry can commence.

We know not the precise physiological conditions incident to this state; but we do know with absolute certainty that during its continuance, if the sleep be natural and perfect, there exists a suspension, in a great degree, of all mental action—of all sensitive and muscular actions—of all the motions, in fine, which distinguish the animal from the plant.

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[*] "We must suppose then, that in health, whenever there is any greater exertion than common (which always increases the pulse), the heart *dilates* more—*contracts* more—and does both with greater velocity. This, I conceive, arises from a necessity which begins first in the veins."—*Hunter on the Blood,* p. 186.

The aggregate amount of vital motions, then, which always proceed in a waking animal, however unexcited, suffers during sleep an abstraction of all such as are unconnected with the organic support of the machine; and as we have already shown the consumption of red blood to be coincident with action of every kind, so, during this state, the aggregate consumption of this fluid suffers also a diminution commensurate with the diminution of action.

What then is the effect of this on the circulating and respiratory organs?

The pulsations of the heart are slow, regular, and measured; the respirations quietly and tardily performed; the demand for arterial blood is small, and the energies of its producing and distributing organs correspondingly diminished.

From this state the animal awakes; in other words, actions before quiescent are called into exercise and superadded to those previously performed; consciousness, as we name it, returns; the brain recognizes impressions transmitted by awakened sense, and evolves corresponding ideas; muscular motion resumes its play, and the sum of action throughout the body is augmented.

We here only exemplify the difference between the minimum absolute quiescence of a healthy living animal, and the smallest extension of its action; but precisely the same phenomena, in an exaggerated degree, present themselves in the ascending scale of vitality.

If the animal just aroused be called on suddenly for an unusual exercise of one or many functions, so much the quicker does his pulse beat, and his respirations proceed. To this extension there are no limits but those dependent on the inherent power of the individual; the acme of vital capability seems to be attained when the heart is no longer able, in the necessary time, to transmit to the lungs the venous blood which the system supplies, or the lungs able to produce the quantity of arterial blood which the system consumes. At this point a temporary —at times a fatal—suspension of all the phenomena of life occurs, apparently as the simple result of their own inordinate exercise.

What then constitutes the difference between high and low action? Why does the heart beat slowly, and the respirations proceed with diminished velocity in sleep? and why do their motions increase as other actions increase? Let us first ask ourselves what corporeal change occurs when an animal awakes?

We have already said that his actions increase in number; but what material change occurs in consequence of this?

The primary effect, and that produced with electric speed, is the consumption of arterial blood in the capillaries of the organ newly excited into action; and just as these newly roused actions are numerous, or energetically performed, so is this effect increased. But red blood cannot be consumed without black blood being produced as a coincident occurrence ; and hence, for every drop of the former changed in the final artery, one of the latter is transmitted into the incipient vein.

Here, then, we apprehend arises the "necessity" to which Mr. Hunter alludes,—a "necessity" which becomes greater as action increases, less as action diminishes,—a "necessity" which constitutes the point from which the subsequent velocities of the heart and respiration spring,—a "necessity" which is the result of *arterial consumption,* and itself dependent on *venous production.*

But the influence exerted by this "necessity" does not here end; being itself produced by an increase in the expenditure of red blood, it in turn leads to an increased formation of the same fluid; for assuming action to remain at the point which first created the necessity, it is obvious that for the continued sustenance of this at that point, the exhaustion caused by it throughout the arterial system must be immediately supplied by an equivalent formation of its proper blood, which the excited capillaries still continue to consume in unaltered quantity.

We have hitherto however only traced this primary cause of action to the radicles of the systemic veins. It remains to be seen how this reacts on the functions of the heart and lungs.

The first remark we have here to make is this, that the contact of blood with the interior of the heart appears to be the *exciting* cause of its action—the second, that the contact of red blood with its minute organization appears to be the immediate cause of the *execution* of this action. The larger the quantity of blood therefore which reaches the heart, the more forcibly and quickly does it act—and the more forcibly it acts, the more red blood is consumed in its own proper vessels, so that thus the organ becomes in part its own stimulator.

But again, the more strongly and quickly the heart acts, the more blood does it emit from each of its ventricles; or, the more red blood does it send to the system through the aorta—and the more black blood to the lungs through the pulmonary artery.

Further, we have every reason to conclude that the quantity of blood emitted by each ventricle in a stated time is equal, so that the lung receives for the purpose of reddening, precisely the same quantity of black blood that the system receives for the support of action. Again, the emission of red blood by the left ventricle and of black blood by the right ventricle, necessarily implies the reception of equivalent quantities in a stated time by their respective auricles; so that just as much venous blood reaches the right auricle from the system, as arterial blood reaches the left auricle from the lungs. This at least appears to occur during the healthy condition of an animal, under all the various conditions of excitement to which it may be exposed.

But, admitting it conceded that the two hearts contract with equal velocity, and transmit equal quantities of blood—still it is probable that the motion of the one is the immediate primary cause of the motion of the other—and the question therefore recurs, Which is the first stimulated? What is the nature of the stimulus which acts on it? How is it increased, and how diminished?

We seem to have advanced a step in the solution of these questions, by showing that action of every kind and anyhow induced, consumes arterial, and, as a necessary result, produces venous blood in quantities proportioned to its intensity; and having on this principle established a necessity varying in degree at the commencement of the systemic veins, and assumed that the material cause producing this necessity stimulates the right heart when brought into contact with its internal membrane; the remaining difficulty consists in showing how this transfer is effected—a question connected not so much with the general physiology, as with the vito-mechanism of circulation.

It has been my desire as much as possible to avoid discussion on the propelling forces of the blood, as by no means immediately necessary to our present purpose. It is sufficient to assume that all the blood which finds entrance into the systemic veins, speedily reaches the right heart; nor is this certainty at all the less because opinions are unsettled as to the cause of its advance.

On every ground however we are entitled to conclude, that the quantity of dark blood which reaches the right auricle is directly proportioned to the anterior activity of systemic action; and as we have equal reason to believe that its corresponding ventricle responds by the force and velocity of its contractions to the quantity received; so it follows, that in every case the pulmonary artery has transmitted through it in a given time just as much venous blood as the systemic capillaries had produced.

Here again we waive a consideration of the question, how the blood which thus reaches the trunk of this vessel performs its circle through the lungs. It may be by a force directly communicated by the ventricle itself—it may be by some dilative and contractile power resident in the pulmonic capillaries—it may be by a similar power in the left ventricle—or what is the most probable supposition, that all those vito-mechanical forces contribute to the production of one result. But however we explain this—the fact is certain, that for every portion of blood which enters the pulmonic artery, an equivalent portion reaches the left auricle, having sustained during its transit that change on which its efficacy as a vital agent depends.

Here then do we find a necessity originally created by the exertion of vital attributes in organs, met by the fulfilment of a reddening process in the lungs; and perceive how action leads remotely and through a long chain of causes and effects, to the reproduction of the fluid by which action is sustained.

We conclude then, that the great final purpose served by acceleration of the heart and respirations, is the production and distribution of an augmented quantity of red blood, necessarily contingent on increased systemic action; and the great cause of their retardation, the diminished consumption of this fluid contingent on moderated action; the primary physical cause regulating variations in their velocities, being the variable quantities of black blood produced at the extremity of the systemic circle, and impelled or drawn by certain vito-mechanical forces to the right auricle.

We should on these grounds be led to infer, that when action increases, and when as a consequence the heart and lungs both perform their functions with augmented speed, the quickening of the heart should be the first step in the process.

We believe it to be demonstrable that this is the case, or at all events that the inference from a very simple experiment nearly amounts to demonstration.

Over the motions of the heart itself we possess no immediate control; we can only act on it indirectly, by increasing or diminishing the force of other voluntary motions which react upon it; and hence the experiment cannot be made by commencing with the heart and observing its reaction on the lungs, inasmuch as all the causes of excitement brought to bear on this organ are such as necessarily induce also augmented respiration.

But, the converse of the experiment may be readily tried, because over the motions of the chest we *do* possess a voluntary control, and can therefore by accelerating its motions spontaneously observe with precision whether this is followed by corresponding quickening of the pulse.

The case appears to stand thus—if we find that quickened respiration always attends on quickened cardiac contraction, it affords no proof of their reciprocal reaction, because in all cases the causes which increase the heart's motions are exactly those which increase also the force of respiration; nor have we it in our power to augment the contractions of the heart without their aid. But if we find that respiration voluntarily increased does not within certain limits accelerate the motions of the heart—then do we appear to be furnished with the best evidence of which the question is susceptible, that cardiac excitement is the *cause* of pulmonic excitement—that the chest heaves rapidly *because* the heart contracts rapidly: and the reverse—a point not readily perceived independent of experiment, on account of the extreme speed with which the one action succeeds the other.

Let us suppose the case of a healthy person of adult age, not subjected to any extraordinary causes of excitement, and assume the pulsations of his heart to be 75 in the minute, which may be considered as about its mean velocity. In such a case, the respirations will be as near as possible 18 in the minute, or maintain a ratio to the pulse of about 1 to 4.

By a voluntary effort, he can now without difficulty double, or more than double, the number of his respirations in a stated time, and yet the pulsations of the heart are little, if at all, affected —proving that an extension of the respiratory function exerts but a slight influence in producing accelerated contraction of the heart.*

Assuming the correctness of the views before taken, we appear unavoidably conducted to this general conclusion, that increments in the velocities of the heart and respirations own as their primary cause the *consumption* of arterial blood, and have for their final purpose the *pro-*

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* In stating that the heart is *little* affected, I mean to say, that it never is so in such a degree as would necessarily be the case, had augmented respiration exerted the same influence in producing increased action of the heart as increased action of that organ otherwise induced, at all times exerts over the motions of the chest; a few pulsations are indeed often added to the previous number of its beats, but this is in strict accordance with the opinions urged. Muscular effort is required for the experiment, and this like other vital acts necessarily accelerates the pulse, but the acceleration is here obviously the effect of increased action of the systemic vessels—not of direct excitement of the heart as a sequel of augmented respiration.

duction and *distribution* of arterial blood. And we conceive that a recognition of this single physiological truth may be applied to some advantage in conducting us to the rational treatment of many diseases—especially of such as directly affect the viscera contained within the chest. With a view to facilitate such application, it appears desirable here to notice some of the causes by which the natural healthy balance between systemic action on the one hand, and thoracic action on the other, are modified.

The body, viewed under the aspect we have considered it, presents a living microcosm, in which millions of capillary vessels belonging to the aortic circulation are engaged in effecting the different actions of life ; and in which millions of others, connected with the pulmonic circulation, are as constantly engaged in supplying the fluid by which the efficiency of the others is sustained.

As the activity of the first class increases, so is it necessary that the activity of the other class should correspondingly augment—or, in other words, as an increased quantity of red blood is consumed, so is it necessary that an increased quantity of red blood should be produced ; and this balance seems to be maintained by an accelerated action of the heart and respirations. By the one, an augmented quantity of black blood is propelled through the lungs—by the other, an augmented quantity of air enters these organs in a given time to meet the demand for reddening—both having for their final purpose, the transmission of an augmented quantity of red blood to the capillary vessels of the system.

As therefore the velocities of the heart and respiration are dependant on the quantity of red blood which the body requires in a given time —so the facility with which the fulfilment of this purpose is accomplished must be presumed to vary from many circumstances—some connected with variations in the organs of supply—and some with variations in the organs of consumption.

Thus, when from the natural form of an individual, the capacity of his chest bears a small ratio to the size of his body ; it would be a matter of rational inference, that under a stated amount of systemic action the pulse and respirations should be unusually quick ; and that they should be unusually slow where the chest bore a high ratio to the body.

Again, we might infer on the principle advanced, that where two persons present as near as may be the same organization, that the one more readily excited into systemic action, would necessarily and habitually exhibit a pulse and respirations of greater velocity than the other, whose excitability (or what means the same thing—*proneness to action*) was small ; or to use familiar words, we should expect that the phlegmatic man, as he is termed, should have a slower pulse and tardier respirations than the man of sanguine or choleric temperament

—assuming of course both to be placed under the same circumstances.

On the other hand, a relatively capacious structure of the organs of production, and a great insusceptibility of action in the organs of consumption, might, if our hypothesis be correct, be assumed as the circumstances under which an habitually slow pulse and slow respirations might be expected; where either condition exists, we would be induced to predict their slowness; where both conditions occurred, we should anticipate the slowest pulse and respirations compatible with a state not properly one of disease.

But if the above be a legitimate deduction as regards persons of natural structure and healthy action, not less legitimate must it be when applied to those whose lungs, from any cause, have sustained such a diminution of their natural capacity, or such a deterioration in their functional attributes, as renders, either permanently or for a time only, the production of arterial blood a work of unusual difficulty; and not less legitimate also must it be towards those, whose lungs remaining healthy, have the actions of their systemic capillaries morbidly increased in such a degree as to render the call on those organs excessive. In the one case we should be justified in expecting an accelerated velocity of the pulse and respiration, because the producing organs of arterial blood are diminished in volume, or impaired in function; in the other we are justified in expecting a similar effect, because the lungs (or producing organs) remaining healthy, the systemic vessels (or consuming organs) make for the time a larger call on their energies; in point of fact, we conclude that both conditions would lead to a common result, though in different ways.

What we have here inferred as theoretically probable, appears to be in strict accordance with the facts which observation determines.

So far as our observation has extended it is a law, quite as well established as what are usually called the *laws* of physiology, that healthy persons who possess a chest relatively capacious, as regards the whole body, exhibit habitually, in reference to existing circumstances of action, a slow pulse and slow respirations ; that in those of contracted chest, the reverse of this occurs ; that where a contracted thorax co-exists with a temperament of ready excitability, the pulse and breathings are always unusually quick ; and unusually slow where a phlegmatic temperament co-exists with a capacious chest.

It appears to me, after considerable practical investigation of this question, that the variable velocities of the pulse and breathing which must on all hands be admitted to exist, are referrible to one or other of the causes named.

By reference to the same principle may we explain those variations in their action which are connected with various ages.

The pulse and respirations of the infant are rapid to an extent which would be morbid at any other period of life; in youth both become comparatively moderated; and in old age the lowest standard, compatible with healthy life, is found. Why do these phenomena occur? On what principle can they be explained?

In one and all, the velocities of the circulation and respiration strictly correspond with the aggregate consumption of red blood in each.

In infancy nutrition is at the *acme* of activity; every capillary vessel rapidly consumes arterial blood, in ministering to the growth of the recent organism; as it advances into youth, the necessity for this is diminished; in adult life no increase is required; nutrition is confined simply to the support of the machine; at this period, however, other actions proceed with augmented force; the sensitive and locomotive functions are in full vigour, and the intellectual powers act with a new energy; hence, any decrease in the consumption of red blood, in consequence of a diminution in the nutritive act, is in some degree compensated by its employment in other vital acts, and the velocity of the heart and respiration is scarcely below the average of youth. In old age not only is nutrition languidly performed, but all other vital motions proceed with diminished energy; a small portion only of red blood is required, and the organs of its production and supply sustain a comparative repose.*

* In thus applying the principle advanced to elucidate the cause of those differences observed in the circulation and respiration at

It is freely admitted that the nature of vital action receives no explanation from these remarks; we have throughout rested on this as a final and inexplicable fact. On what quality of living matter it depends, that its motions should be extremely active in infancy, moderated in youth, stationary in adult age, and slow as life approaches to its end, are questions which physiology cannot answer; but we still conceive that it constitutes a curious, and as regards disease, an important object, to trace to their source the causes of those variations induced by action on the motions of the two vital organs on which life appears immediately and necessarily to depend.

But we have hitherto, in our attempts to elucidate this question by a reference to facts readily observed, confined our observations to healthy states alone; it remains yet to be seen, how far certain phenomena connected with morbid conditions are explicable by reference to the same principle.

We have already stated our conviction that habitual velocity of the pulse and respirations are referable in health to one of two causes,—

various ages, it is of course implied that at each period of life the action of those functions is modified by the causes already or hereafter to be named. Thus, an infant has always a quick pulse, but *that* infant has the *quickest* whose chest is relatively small, or whose organization is unusually excitable; an old man has nearly always a *slow* pulse, but *that* old man has the slowest whose chest is unusually capacious, or whose temperament is exceedingly phlegmatic. We thus consider *age* as *one*, but *one* only, of the many circumstances which modify the habitual velocities of the heart and respiration.

First, Pulmonic surfaces small in reference to the aortic surfaces.

Secondly, A readiness of the latter to acquire action from the operation of slight impressions: a tendency usually expressed by the term excitability.

When either of these circumstances is present, the effect we have stated seems to follow; when both co-exist in the same individual, the velocities of the heart and respiration attain the highest point compatiblé with a state of health.

Now it would appear that various morbid conditions must be considered as placing the body under circumstances essentially analagous to those hitherto named, so far as their influence on the respiration and circulation is concerned; and that hence the accompanying phenomena morbidly manifested by those functions are susceptible of elucidation to some extent, on the very same principle which we have already applied to this purpose in healthy states.

It may be set down as a proposition subject to few if any exceptions, that the functions of the heart and lungs are in a greater or less degree interfered with in all morbid conditions; nor does it appear less true that, in the large majority of instances, this interference is manifested by an exaggeration of their healthy motions.

The exceptions to this very general rule seem to be found in those maladies which have a direct tendency to diminish action by influencing the source from which, in a large degree, action springs; and we consequently find that diminished velocity of the functions named chiefly attends those diseases in which the nervous centres are involved. The slow pulse and measured breathing of perfect apoplexy, the weak pulse of paralysis, or the nearly imperceptible one of cyncope, offer ready illustrations of this truth.*

But in the great majority of diseases, among the other phenomena they present, we find them characterized by an exaggerated action of the heart and breathing, and we have hence to consider on what principle this is to be explained in accordance with the view adopted.

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* I am aware that there may seem a contradiction in what is here advanced, when compared with the opinions expressed at page 22. Our leading object throughout has been to show, that vital action of every kind is dependant on red blood—that the change of red blood to black is in all instances the coincident of action; and that hence, in proportion to the amount of action performed, so is the quantity of red blood consumed; and so the necessity for its production originated.

But, the objection may be made that we abandon this on admitting, that action is less as nervous power is less—thus apparently conceding that it depends not on the *blood*, but on the *nerves*.

I must again, in reply to this, at first sight, specious objection, request the attention of my reader to the distinction drawn between the "*excitation*" and "*execution*" of a vital act. I have stated that, in my belief, nervous power, to a large extent, induces action, but that the contact of red blood with the extremity of the nerve executes it; and hence it is quite consistent with such theory, that where nerves are not in a condition to induce, there willbe less action in the aggregate, and consequently less consumption of the executive of action. Were it admissible to compare things in themselves dissimilar, or institute an analogy between the body natural and the body corporate, I would say, that *nerves* bear the same relation to *arteries*, which the *legislative* body of a well-regulated nation bears to the *executive*—the one dictates action, the other executes it.

Here, again, we renounce all attempts to determine either the nature of morbid action, or the mode in which various exciting causes operate in its production. Why it should happen that a stimulant applied to a living part should increase the exertion of its vital attributes—why certain miasmata or contagious matter, aerial or otherwise, should induce an extention of action in the whole capillary vessels of the body—why certain agents should first irritate and then inflame the textures on which they act—or why the inflammation so induced should run a particular course and lead to particular results, are, one and all, questions we do not attempt to explain, but we feel justified in assuming, that, whether causes of this kind act on the body generally, or parts of it only, they lead for the time to an augmentation of vitality; that in so doing they, as an inevitable consequence, increase the consumption of red blood at the circumference of the aortic tree, and thus originate a necessity for equivalent production at that of the pulmonic tree.

Now the morbid actions which give rise to such effects may be infinitely various in their intensity and seat, but so far as they bear on the question immediately under discussion, they appear susceptible of arrangement under the three general heads of causes which induce

1. Irritation.
2. Inflammation.
3. General Fever.

To one or other of these classes may be referred all causes of acceleration of the organs of respiration and circulation which act by inducing a morbid excess of motion in the vessels which terminate the aortic system.

To illustrate this, let us in the first place ask ourselves what definite idea we attach to the term "irritation"? We conceive it to be definable in one way only—an exaggerated, and therefore uneasy, action of that function of animal life which we name general sensibility ; but we have already endeavoured to show that the act of a nerve which produces sensation takes place only under the contact of arterial blood—that a certain amount of sensation being produced, a corresponding amount of red blood is changed ; and that hence the greater the quantity of sensation elicited, the greater also must be the consumption of red blood, and the greater the necessity established for its production. Here then do we appear to arrive at some definite conception of the reason why the heart and breathing are excited by causes acting on the extremities of sentient nerves. It does not indeed explain how the irritating cause produces its effect—still less how an extension of the sensitive action of a nervous fibril should also lead to an extension of action in the capillary artery in connexion with it; but assuming that such reciprocity does exist, it leads us, we apprehend, to perceive through what changes actions of this nature occurring in a part, lead unavoidably to an augmentation of those vital functions from which they sprung, and the slower or quicker motions of which they subsequently determine; and we further submit that it does so in a manner much more satisfactory than were we to appeal to the vague notion of "sympathy," which, employed in any other sense than simply as the expression of a fact, is, of all others in physiology, the most unsatisfactory and the most obscure.

If, again, we attempt to illustrate this general proposition by a reference to the common phenomena of inflammation, it is certainly true that the essential nature of this condition is in no way elucidated on the theory it has been our object to explain; we are still left in ignorance why any cause acting on healthy structures should, in the first place, augment the actions of which their vessels are naturally susceptible; and next, by a still further excitation of such vessels, lay the foundation for a series of changes in many respects different from those which they succeed. Our theory does not indeed touch the question why "irritation," as we term it, first excites the capillary arteries;—why, if intense, or long continued, it next stimulates them into the condition which we term *inflammation;*—or why this state, being once established, tends to the production of suppuration, or ulceration, or any other of its numerous sequelæ.

But, whatever difficulties those recondite and often-agitated questions may involve, we hold it to be a fact readily demonstrable, that in the early stage of inflammation an increased quantity of blood in a stated time passes through the capillary vessels of the part; but the blood which thus flows in additional quantity through the arteries is still red; and that which returns in additional quantity by the veins is still black; there is consequently something more connected with this process than merely an increased transfer of blood through a part—and in what does this consist?—obviously in the augmented abstraction by its vessels of that indeterminate matter which constitutes the difference between red and black blood—of that without which vital motions cannot be effected. And again, what consecutive acts does this involve? It clearly engenders a necessity, *originating in the part itself,* which induces an augmented action also of the organs by which *red blood* is formed and transmitted.

If, again, we advert to the phenomena of simple or idiopathic fever, similar effects on the functions of the heart and lungs appear to be susceptible of reference to a cause which if not identical, is at least analagous.

We will not indeed attempt to maintain, as some pathologists have done, that fever is nothing more than general inflammation, although the two morbid states undoubtedly present phenomena of striking similitude. Neither do we feel disposed to agree with others who endeavour to connect the symptoms of fever with a local cause. On the contrary, we hold no point in pathology to be established on better evidence than that very many causes are capable of exciting the whole capillary vessels of the body into the preternatural actions which we express by the name of fever, without any one part being necessarily more affected than another. This however involves a question in general pathology alien to our present purpose; but whether we consider the exciting causes of this disease to operate generally, or whether we consider their influence to be first directed to a part alone, and thence propagated to the whole system of capillary vessels, it would appear, that in the early stage of fever—and until the system becomes partly worn out by the abnormal activity of its own actions—a larger quantity of blood in a stated time passes through the capillaries; but here also as in local inflammation, there is something more than a simple increase in the transit of blood, for the arteries still convey red blood, and the veins still return black; from which it follows, that in the one case as in the other, an unwonted abstraction has occurred of that quality which constitutes the distinction between them.

In the view thus taken of the causes of accelerated vascular and respiratory action, it would appear that the same effects are frequently induced by causes differing in their nature—but which produce those by effecting a similar change on blood, the great moving agent of vitality. Irritation, it is true, differs in its nature from inflammation, as inflammation probably differs essentially from fever, though often co-existent with it; but one and all of those morbid actions are strictly associated so far as regards their one leading result—the augmented consumption of red blood at the extremities of the aortic tree, and the augmented production of that fluid at the extremities of the pulmonic tree; the heart being the great central director and distributor of the fluid on which those changes are effected, and by which they are produced.

In this respect we apprehend there exists a close analogy between morbid conditions and a simple exaggeration of healthy action. High mental excitement, extraordinary muscular effort, and many similar causes which induce an unusual exertion of normal actions, appear to produce similar results; the leading difference between them seems to consist in this—that the mere extension of a natural action is generally of a voluntary, always of a temporary, character, and destined to meet some particular emergency; while an extension contingent on disease is of undefined duration, —uncertain in its results, and in no case under the control of the individual either in its origin or end. Yet this does not appear in the least degree to affect the force of our fundamental proposition, that, in either case, the *cause* of acceleration in the heart and lungs has its seat in the intimate organization, and produces its *effects* through the fluid medium, in the way already sufficiently detailed.

But again, increased action of the heart and lungs, though it always accompanies increased action of the systemic vessels, is clearly not dependant on it alone: so far from this being the case, we often find a great acceleration of their functions present, without the existence of any evidence by which an unwonted action of those vessels is indicated.

Here, then, we must look for some other cause leading to the same result—some reason capable of explaining how the central organs, which form and transmit the red blood, should, in certain cases, act with unusual energy when there appears no extraordinary demand for this fluid.

The remarks previously made on the balance of action maintained between the pulmonic and systemic vessels, will, in some degree, have paved the way to this explanation.

If, as we have shown, all increments of systemic action in a normal condition of the lungs, are necessarily accompanied by corresponding increments of pulmonic and cardiac action, not less obvious does it seem that, assuming the lungs to be in such a state of disease as limits their reddening surfaces, a stated increase of systemic action will produce on them a greater effect than when this is not the case; or, to put the argument in a different form, that existing states of systemic action, which in the one case lead to no apparent excitement of the heart and lungs, may in the other induce an acceleration of their functions which is altogether unnatural; thus giving rise to a condition often—though in our estimation erroneously—considered as a state of fever.

Thus, assuming an example in which the body and lungs present a condition both dynamically and structurally perfect; and supposing the actions, or rather the capacity of action, in each to be represented by the No. 2; then all increments of such action in the former will be so attended by increments in the latter, that when the one increases to the extent of 4, the other will increase to 4 also; or they will continue to balance each other in accordance with the due and natural relations of a healthy body.

But again, let us suppose a case in which, from the obstruction or consolidation of the pulmonary tissue, the healthy relations between the lungs and the system are so subverted that, while No. 3 represents the latter, 2 only may be presumed to represent the former: then it will follow that, under all states of systemic excitement, the capacity of the lungs to meet this will be relatively $\frac{1}{3}$ less than in natural conditions; thus inducing a deficiency in the supply to the demand, which requires to be compensated by some other arrangement.

It would appear that this is accomplished, under such circumstances, by an accelerated transmission of the blood through those portions of the pulmonary artery which ramify on parts of the lung still permeable to air; that what is deficient in extent of pulmonic surface is thus made up by velocity of motion, and that hence, within certain limits, the system continues to receive a due supply of red blood though at the cost of an unwonted action of the heart and lungs.

We here then recognize a cause of quickened pulse and respiration altogether distinct from that connected with increase of systemic action—a cause which has its origin not in an unwonted expenditure of red blood, but in an unusual impediment opposed to its production.

To such may we reasonably refer that morbid acceleration of the pulse and breathing always present in cases where, from any cause, portions of the lung have become impervious to air; as well as the unusual celerity with which the velocities of both functions are increased, in persons so circumstanced, on every trifling extension of action, either mental or corporeal. The quick panting respiration of consumptive persons on ascending stairs or otherwise exerting their muscular powers—the dispnœa which, even in a state of quiescence, is displayed by people with effusion of water into the chest, or whose lungs are consolidated by inflammatory action—and the distressing increase of this under the most trivial corporeal excitement—are all referrible to one cause, namely, the extreme difficulty with which the sustaining fluid of their actions is produced.

And here a very interesting, and as it would seem, uninvestigated question presents itself.

As we must presume arterial blood to acquire in, and carry from, the lungs something which is the cause of action, so it is possible that pulmonic conditions may exist which, not only present obstacles to the formation of red blood in *natural* quantities, but also *imperfectly* form that which is produced.

Assuming for a moment this to be so, the effect must follow—that, the material which supports action being supplied to the systemic capillaries in diminished quantity by a given amount of blood, a greater proportion of this imperfect fluid will be consumed by them in effecting a stated amount of action; on which supposition there will be established, at the extremities of the aortic tree, an additional cause for that increased velocity of the heart and respirations which owe their origin to abnormal conditions of the lung.

A question of this nature can only be settled by an appeal to anatomy, and by investigations directed to many cases of pulmonary disease which vary in kind and degree. If by these it should be found that there exist morbid states of the lung, in which the pulmonary artery continues to circulate its blood through portions which the air no longer enters, then, unquestionably, a material cause (dependant on a vitiated state of the arterial blood) would be established sufficient to account not only for the destructive influence exerted by chest complaints considered as a class, but also for the varying intensity of this dependant on varieties in the nature of the local affection. To illustrate the idea thus thrown out by an example; let us suppose a case, by no means unusual, in which $\frac{1}{6}$ of the entire lungs has, from any cause, become impervious to air; then, if the pulmonary artery still circulates blood through such portion in common with others, it is obvious that there must arrive at the left side of the heart, for distribution to the system, a mixed arterial current, $\frac{5}{6}$ of which consists of blood properly reddened, and $\frac{1}{6}$ of that which has not undergone the action of the air; nor is it less obvious that, in such case, the systemic capillaries will be supplied with a fluid deficient by *one-sixth* in that quality on which we know their efficient action to depend.

What is here set down as a matter of supposition, I have endeavoured practically to investigate, so far as opportunities presented themselves, by injecting the pulmonary circulation of lungs in different stages and conditions of disease—the only method, as it appears to me, by which this question can be in any degree determined. The result has been that in some morbid states of the lung the pulmonary vessels, or, to speak more correctly, their final or working extremities, appear to become obliterated, coincident with the obliteration of the air tubes or vesicles; while in others, the artery occasionally ramifies over portions which are no longer pervious to the air.

I shall have occasion to allude more particularly to these anatomical facts in the succeeding parts of this treatise; here it is only necessary to observe that they appear to offer grounds for a rational physiological inference capable, in some degree, of accounting for the intensity of the symptoms which results towards different functions of life—especially the nutritive, from some organic lesions of the lung not always commensurate in magnitude with the functional evils they induce.

It would appear then, from all which has been said, that the varying velocities of the heart and lungs, and the varying celerity with which the functions of these organs are augmented in different individuals, are attributable to one of two causes, each dependant on the relations in which the lung stands to the system as regards either extent of surface or capacity of action. In one case the pulse and respirations are always relatively quick and easily accelerated, because the systemic vessels, either from an inherent excitability to natural action from slight causes, or from morbid increments of action, as in fever, inflammation, &c., consume, in a stated time, a relatively larger quantity of red blood than the lungs, without undue action, can create, or the heart, without undue action, distribute.

In the other case these functions are unusually readily accelerated, because without the existence of any undue extension of systemic action, the pulmonic surfaces, whether from limitation of surface consequent on disease—or from presenting a natural structural disproportion to the body—are incapable of meeting, without inordinate increase of their functions, such quantity of red blood as the system consumes.

Finally, we consider that there may exist a third cause of morbid acceleration of the functions alluded to, having its seat partly in altered organization of the lung, and partly in a changed condition of the arterial blood consequent on this, by which that fluid is so deteriorated in its vital properties that a larger quantity is demanded to sustain a stated quantity of action than when it is presented to the aortic capillaries in a perfect state.

The pathological and practical application of the views advanced—more particularly as they bear on the disease which forms the leading subject of this treatise—it will be my endeavour in its succeeding chapters to explain.

The preceding pages are confined to the illustration of one leading proposition—that, as a matter of fact, the consumption of red blood in the arteries of the system is strictly regulated by the existing energy of action.

In thus viewing the question we have nearly abstained from entering on any examination of the causes, mechanical or vital, of the blood's progression, merely assuming such to be sufficient for the purpose, and limiting our observations as much as possible to the one broad view which relates to the balance between the formation of red blood at the extremity of one system of vessels, and of black blood at the extremity of another system.

Here perhaps we might have quitted the subject, as sufficient for any practical applications made of it in the succeeding pages, but further consideration induces the belief that a short notice of *one* of the causes which appear to influence the transmission of the red blood, may serve to render our conception of this matter more definite and distinct.

The idea which originated with Harvey, and has to a large extent actuated the views of physiologists since his day—that the propulsive energy of the heart would account for the transmission of blood *from* that organ back to itself again, is so irreconcilable with many phenomena of the circulation that few if any are now inclined to concede its truth.

The difficulties however of explaining obvious facts on such an assumption are by no means so great in the arterial as in the venous system. We may easily reconcile our minds to the possibility of a central propelling force, distributing the blood throughout all the system of arteries in immediate connexion with the muscular cavity in which such force resides, were it not for certain phenomena which appear to render even this improbable.

Admitting however, for the moment, that the heart, and it alone, may be capable of transmitting the blood throughout the arteries, it is still not easy to conceive that it should have power to do so likewise through the veins, and thus re-supply itself with the same fluid it emits. It is this last difficulty which modern physiologists have chiefly busied themselves, in explaining; they accede in a considerable degree to the opinion of Harvey, as regards the cause of transmission through the arteries, while that of its progress in the veins has engaged a large share of their attention.

We cannot here notice all the arguments advanced, or experiments instituted, to determine the accessory causes of the venous flow; suffice it to say that the principle on which every explanation rests is that of a central vacuum, or rather *tendency to one,* somehow formed either in the heart itself, or in the cavity where this viscus is placed.

But however well founded such explanation may be as regards the transmission of blood in the central direction, it bears little, if at all, on its flow from the heart to the circumference; and however simple it may appear to attribute this exclusively to a central force, yet we apprehend there are points here also which cannot be explained by a reference to this cause alone.

The leading, and in our opinion insuperable, objection to such a view depends on the fact that different quantities of blood evidently pass at the same time through different arteries, and that not in quantities determined by their size, but much more it would appear, in correspondence with the existing activity of action in the parts which they supply. If from any cause the action of an organ be exaggerated, the minute branches, as well as trunk of its artery, transmit an augmented quantity of blood, and this without any accompanying increase in the supply transmitted by the arteries of other parts, whose actions are for the time more quiescent. The obvious determination of blood to the head from protracted mental effort, or the stimulus of excited passion—the additional flow to a part when its muscles act with vigour, or to a gland when its secretion is increased, or to the stomach during the process of digestion, afford common but decided illustrations, nor can they be at all satisfactorily explained on the assumption that red blood reaches the capillaries solely by an impulse received from the heart; on the contrary they appear to inculcate a belief that the causes which, by increasing action, determine increments of arterial excitement, act immediately on parts themselves—that in *them*, consequently, resides an attractive force of some description which, exerted with increased or diminished energy, regulates the amount of red blood which they receive.

A question therefore arises—of what description this attractive force may be, or in what manner does it establish in living parts those physical conditions necessary to regulate the flow of a fluid through living canals.

Here, as in all discussions of the kind, we must commence by assuming the existence of a vital force whose exact nature we neither can comprehend nor conjecture, and merely hope to acquire some idea of how this operates in producing the physical changes on which many phenomena connected with life, and having the support of life as their ultimate purpose, depend.

The final vessels of a living body are too minute to be brought under the cognizance of unaided sense, but when examined by the aid of a strong microscope we at least acquire a knowledge of the fact—that the transmission of blood is accompanied by an alternate contraction and relaxation of the vessels along which it flows.

The force by which these alternate motions are effected is we must assume *vital,* and hence *inexplicable,* but so long as it continues to act, it must, as it appears to us, necessarily induce a mechanical state which, according to the force with which it is exerted, will favor more or less the entrance of red blood into the part where it has its seat.

This condition we conceive to be the occurrence at each motion of each minute vessel of an entire or partial vacuum within its cylinder, and when we come to consider that even a small artery divides into many thousand branches, it is readily conceivable how the aggregate influence exerted by those should largely contribute, on physical principles alone, to the progress of the red blood, however small the power exerted by only one may be.

There are two ways in which this tendency to a vacuum may be produced within the cavity of the capillary vessel.

We may on one view conceive that a dilatation of its parieties takes place in consequence of a *vital capacity of elongation* resident in the fibres which form its coat. On such a supposition the assumed vacuum is caused by an active exercise of living power which having sucked or drawn blood towards the vessel, is again obliterated by a succeeding collapse consequent on the suspension of the force by which it was formerly dilated.

Or, on the other hand, we may consider the *contraction* of the vessel as the vital act; the dilatation which produces our assumed vacuum being merely the return of the vessel to its natural physical state when the act of contraction ceases.

The analogy of the larger arteries would go far to confirm the truth of this last supposition. Patency, without doubt, is their natural physical condition—nor is this fact the less decided whether we *do* or do *not* admit contractility to be in the living state, an attribute of their coats.

If then we apply this analogy to the minutest vessels whose contractile power is on all hands admitted, and presume that patency is here also the natural state—then can we readily conceive how by physical expansion on the one hand, and vital contraction on the other, those alternate conditions may be produced on which some portion of the blood's flow depends. While, then, in the earlier part of this chapter, we have dealt only with the broad fact that every extension of action in a part or in the whole—whether natural or morbid—leads to a corresponding increase in the quantity of blood changed from red to black; we appear, in prosecution of the subject, to be capable of assigning a probable cause for the augmented flow of arterial blood towards parts in which this increased action for the time resides. On this view we consider as the basis of the whole argument that every vital act is strictly dependant on the capillary vessels of the aortic tree, and that their action consequently augments in a corresponding ratio with the extension of each function; how the exciting causes of this extension operate in exalting the vitality of parts we do not attempt to explain, but simply assume, on probable grounds, that it is associated with an increase of that alternate contraction and relaxation of each minute capillary, producing a vacuum within its cylinder which, on physical principles, draws or sucks the blood towards it from the larger branches in connexion, thus finally influencing the motions of that fluid throughout the entire system of the arteries.

On such a principle alone do we conceive it possible to account for the either entire or partial vacuity of the arteries after death.

We must on any view conceive, that the last contraction of the left ventricle propels a certain quantity of blood into those tubes, and hence, on the supposition that the blood is moved through them by a "*vis a tergo*" only, the difficulty remains of showing how their contents are disposed of when this force has ceased to act; for on such grounds it would be the more probable, nay unavoidable, conclusion, that these canals should remain full, just as a pipe through which a forcing pump transmits a fluid remains full, when the piston ceases to act. This difficulty might indeed be got over by assigning to the large arteries a very high degree of contractile force,—but the emptying of their cylinders, even on this assumption, could only be explained, by admitting a contractility sufficiently powerful to obliterate entirely those canals—an extension of the attribute which is scarcely maintained even by the most zealous advocates for its partial existence.

As then the extrusion of the red blood from the interior of its containing vessels after death appears to be inexplicable, either by a reference to the contractions of the heart, or of the larger arteries themselves, we are driven to adopt some other explanation, and none appears so probable, as that the cause of the phenomenon resides in the capillary vessels, the physical influence which they thus exert being dependent on the vacua produced by the mechanical resiliency of their coats, which succeeds to each contraction.

The capillary vessels of the aortic tree are, as Mr. Hunter emphatically names them, the true "workers" in the machine, and the red blood is not only the material on which they work, but by which they are enabled to work at all. So long as the large vessels which supply them contain any portion of this fluid, their *work* still proceeds, they still seem to *draw* towards them, by an inherent power, their stimulus to action, nor can death under ordinary circumstances be said to occur, till every portion of red blood existing in the arterial system, after the last motions of the heart and chest, has been consumed.

Nor is the existence of this vito-physical influence less to be inferred from cases the converse of such as have been already noticed—namely, from those more rare examples in which the arteries continue full after life has ceased.

Some causes of death appear to operate by suddenly checking that innate action of all parts and textures which constitutes vitality ; this is particularly exemplified in examples of poisoning by concentrated prussic acid ; and not less so where death follows instantly on strong shocks of the electric fluid.

In neither case do we know distinctly how the effect is produced, but are driven to refer it to a sudden and entire exhaustion of what we somewhat vaguely term vital power, and along with this an immediate cessation of capillary action. In such cases the larger arteries continue filled with blood; the portion of this fluid impelled into them by the last contraction of the heart, remains stagnant in the cylinder. How shall we account for this phenomenon, except by supposing that in such cases the cause of death at once annihilates the action of the minute vessels, and thus abstracts the force which usually (even after the more marked phenomena of life have ceased) still continues to draw towards its extremity whatever portion of red blood each larger artery may contain.

It appears to us therefore probable that, as the tendency to a vacuum produced within the thorax during the act of inspiration, or in the auricles of the heart after each contraction, powerfully co-operates in the return of the venous blood to that organ, so do the contractions and succeeding dilatations of the unnumbered capillary arteries of a part, aid essentially in the flow of blood *from* the heart towards the circumference.

On this view we find existing at each end of the systemic chain of blood vessels, a physical cause in constant operation, produced indeed by vital effort and having the support of life as its final purpose; but *when* so produced, mechanical in its nature and influencing the flow of blood on mechanical principles alone.

It is thus, that two of the great moving powers of nature not unfrequently co-operate in the production of a common result. In the example before us, *vital* energy induces the *physical* condition with which the support of life is in a large degree connected; a capillary vacuum is created, the blood consequently flows onwards, impelled by atmospheric pressure; and the result obtained is the reception by the organs of a fluid on which vitality depends.

The admission of a physical cause for the blood's motion, seated at the ramified extremities of the aortic tree, is quite compatible with still assigning to the heart a great share in the circulation; it seems probable that the large arteries are chiefly filled by the contraction of the ventricles, a continuous stream of blood being thus constantly ready, between the root of the aorta and the commencement of the really working vessels, which at once supplies any demand that those may make. The force exerted by them is indeed on this supposition rather an emptying than a filling force—it abstracts blood from the vessels, and by so doing, indirectly causes them to be again supplied; while at the same moment by creating venous blood—an act always coincident with the exhaustion of arterial, it lays a foundation for a continuance and renewal of those central acts of respiration and cardiac contraction by which the red blood wanted is again transmitted.

In bringing this preliminary chapter to a conclusion, it is proper to add, that I am far from assuming originality in the views which it has been its object to maintain.

The independence of the capillary circulation was long since advocated by Bordeu, and forms an important feature in the doctrines so well illustrated by Bichat, in many respects the most gifted physiologist of modern times; nor should I here fail to notice the name of Sir C. Bell, who, in a small Treatise on the Circulation, published in 1819, points out with his usual acuteness, the insuperable difficulties which accompany all attempts to connect increments of local arterial action with preceding augmented energy of the heart. Mr. Hunter, also, whose original views on every point connected with the blood and its vessels, constitute him the great physiologist, not simply of his day, but of all time; when he conjectures that a "necessity" for augmented action of the heart and lungs commences in the veins—points with great distinctness to the leading idea which I have in these pages endeavoured to enforce.

Finally, the assumption of a moving force of the red blood resident at the circumference of the arterial tree, seems involved necessarily in the doctrine of Bichat, though by him its mode of action is not very distinctly defined. In the writings of Dr. Pring, this force is alluded to in terms so express, as to induce Dr. Mason Good to assign it the name of "*vis a fronte*," in distinction to the "*vis a tergo*," applied to the heart's action by the great Harvey and his successors.

After all the reflection, experiment, and observation, which has thus been directed to the subject by men great in physiology, it would indeed be presumptuous to hope, that any new idea had occurred to myself, on a question which has already undergone so much discussion: but I put it to the candour and judgment of every physiologist of the present day, whether the mass of thinking which has thus been directed to the elucidation of an important function, influences, as it ought to influence, the objects of our art? I ask him, whether the student, who attends our lectures, or leaves our hospitals, generally goes forth into life and into practice with any other idea than that the heart is the great cause as well as great regulator of the circulation? whether he usually possesses any very precise conception of the relations existing between the heart and lungs—of the immediate causes inducing variations in their action—or of the final purpose which such variations serve? Above all, I ask whether, assuming him to possess correct notions on the subject, those have as yet been made the basis of direct practical induction as regards the treatment of disease? And having asked those questions, I found, on the anticipated answer, my apology for this attempt to aid in the establishment of a simple physiological principle, capable in some measure of redeeming us from that empirical practice which is too frequently observed in the treatment of the formidable malady which constitutes the main object of the present treatise.

CHAPTER I.

PROPRIETY OF DEFINING TERMS—THE WORD PHTHISIS LIMITED TO EXPRESS ONE DISEASE OF THE LUNG—ITS PHYSICAL CAUSE—PRESENT OPINIONS ON THE NATURE, SEAT, AND MODE OF PRODUCTION OF TUBERCLES EXAMINED—CONCLUSION FROM THIS—PROPOSED ARRANGEMENT OF THE SUBJECT.

Few circumstances have tended more to obscure the writings of medical authors than a want of precision in the terms they employ, and to no case does this apply with greater force than to pulmonary consumption.

The older writers on this disease deduced their idea of its nature almost exclusively from symptoms, of which cough, expectoration, hectic fever, and emaciation of the body, were those more prominent and constant. When such existed, consumption or phthisis was assumed to be present, and this conclusion was arrived at without any due consideration of the important fact, that several morbid conditions of the lung produce similar phenomena, and without much attention being paid to the special local states of these organs of which such phenomena were the result. But, as it came to be observed, that such manifestations of disease were in reality connected with very different conditions of the lung, more precise views began to be entertained, and the word phthisis was employed to express the character of a genus including many species; such being founded sometimes on a difference in the exciting cause, and sometimes on the particular nature of the local lesion on which the symptoms depended. In this mode of viewing the question, the idea implied in the general term was still derived from an assemblage of symptoms, its species being founded on the causes—real, or presumed—of which they were the effects. The system of Sauvages presents us accordingly with no less than twenty species of consumption; and did we pursue the investigation into other nosological arrangements, a similar principle would be found to pervade them all; although among practical writers the division has been more restricted, the tuberculous, catarrhal, and apostematous varieties being such only as are usually recognised. As morbid anatomy attracted more attention, a tendency arose to limit the term phthisis to a defined condition of the pulmonary textures; but even Bayle, the earliest of modern writers who minutely describes it, still adhered to the older method of dividing this formidable malady into various species; though as Laennec has well remarked, his division is rather apparent than real; the varieties relating more to different modifications of the same pathological state, than to any actual difference in the nature of the local lesion.*

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* Bayle recognises six species of consumption, the *granular, tuberculous, cancerous, melanotic, ulcerous,* and *calculous.* On

It is to this latter author, however, that we are indebted for restricting the term phthisis,* almost entirely, to express a condition which results from tubercular deposit, and the advantages of this have been so strongly felt, that few medical persons now apply the word in any other sense. In conformity with this view, I should wish the following pages to be considered as referring exclusively to a disease which manifests its presence by an assemblage of symptoms of which cough, impeded respiration—especially on muscular exertion, emaciation, hectic fever, and expectoration in the later stages, are the more prominent and constant; such symptoms being the result of an impaired physical condition of the lungs, in consequence of a deposition in their substance of certain adventitious matter of a whitish yellow colour, and cheesy consistence, whose particles coalesce into masses of variable magnitude, named *tubercles,* increasing progressively as the disease advances, and exhibiting a strong tendency to pass into a

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this arrangement, Laennec remarks, "That the first is a mere variety of the tuberculous; the third, is the partial gangrene of the lungs formerly described; and the three others, are affections which have nothing in common with the tubercular phthisis, except that they have their seat in the same organ." Whether Laennec is right in thus assuming "granulations" to be tubercles in their early stage, may fairly be questioned; but certainly he is so in the general comment on Bayle's arrangement.

* "This" (tubercular), "I think, is the only kind of phthisis which we should admit, unless indeed it were the *phthisis nervosa* and the chronic catarrh simulating tuberculous phthisis."—*Laennec on Diseases of the Chest.—Forbes's Translation, p.* 271-2.

softened state and become removed by the act of coughing—the result being the formation of abscesses, vomicæ, or cavities; all which names have been indiscriminately applied to the excavations which are left behind.

It is certain, that in reflecting on a question of this nature, with a view to the treatment of disease, our investigations ought to be directed to the cause, of which the symptoms are an effect. If in our attempts to remedy tubercular phthisis we only attempt to combat each symptom as it occurs—if we merely palliate the cough by anodyne or narcotic remedies—or facilitate expectoration by the use of specifics presumed to possess such a power—or endeavour to meet emaciation and debility by the use of nutritious diet, which the impaired assimilative function is too frequently unable to concoct—or by stimulating remedies which usually induce a subsequent depression greater than any temporary benefit they confer; and if, while employed in such attempts, we neglect the physical cause of all these symptoms, and the physiological mode of its production, there seems no need of argument to prove that however we may mitigate, we can scarcely hope to cure. On the contrary, our endeavours require to be directed to the nature, causes, and seat, of the foreign matter of which tubercle is composed; to the particular texture of the lung in which it is deposited; to the primary link in the chain of morbid causes and effects which in a secondary manner leads to local deposition; to the establishment of some rational and consistent theory, on which this may be explained, and from which some practical inductions may be made, of means calculated either to arrest the tendency to deposition, or remove the matter deposited, after this has taken place. Nor have investigations of the kind been by any means neglected, though I shall probably be borne out in the assertion, that they have not hitherto led to any very decided results; and that at this moment opinions are quite as unsettled as to the seat and origin of pulmonary tubercles, or to any rational theory directed to their removal, as they were before the valuable labours of modern pathologists had thrown so much light on their physical characters, and the means of detecting their presence in the living body. Those who are acquainted with the existing position of this question, will probably admit the general truth of these remarks; but as it seems desirable, in dealing with a subject of this kind, to possess a clear conception of the point from which we set out, I purpose to offer a few observations on the opinions, as regards tubercle, which now prevail, and the arguments by which each is supported.

I abstain from carrying the reader back to the older opinions on this matter, as far too hypothetical and vague to convey the least information; it is indeed easy to talk of tubercle as "stagnated" or "corrupted" phlegm, or as a "crude humour," or to employ other similar expressions, which in truth convey no real information,—but merely feel desirous of laying before him, a brief notice of how the question at present stands, as gathered from the recorded opinions of modern pathologists.

Those opinions, frequently varying in minor points of detail, may be classed under three heads :

According to the first, tubercle of the lung is the result of inflammatory action.

According to the second, it depends on some morbid condition of the function of secretion.

According to the third, it is the result of some error in the process of nutrition.

Let us briefly examine whether these views afford a satisfactory explanation of the morbid process they profess to elucidate.

1. *Is tubercle the effect of inflammation ?*

The quick pulse, hurried breathing, pain located in the chest, and other similar symptoms which are frequently present in cases of consumption, being all such as usually attend on inflammation of the lungs, very naturally suggested the idea that this state formed the first step in the series of morbid actions by which tubercle was evolved; nor did this idea fail to receive some confirmation from the strong analogy which the new matter presented to the lymph so frequently poured out as the sequel of inflammatory action.

A belief, therefore, in the connexion between those two morbid processes, existed at a very early period; Galen, and many of the ancient writers, appear as its advocates; and it has since continued to maintain its ground, throughout the long list of systems and of schools, between which the medical pendulum has vibrated, down even to the present day.

In more recent times its advocates are by no means either few or undistinguished; of British authors, Dr. Alison, one of the most sound physicians of his day, has strongly advocated the opinion: and in France, the ingenious Broussais, whose universal system of disease is in a great degree founded on inflammatory action of the vessels, has applied it with much zeal to the elucidation, or supposed elucidation of phthisis.

It may appear strange that a controversy of this nature should so long continue undecided; that the direct connexion between the two conditions should not yet have been clearly pointed out; but when we come to analyze the merits of this question a little more minutely, the wonder at once vanishes.

It is admitted on all hands that inflammation is very frequently present in cases of tubercular consumption, but the really important question still recurs, whether the deposition of tubercle precedes, or succeeds, the inflammatory state which it so frequently accompanies? for admitting the very usual co-existence, this in no way proves their etiological relation, and the adherents of either opinion consequently join issue at this point.

As inflammation of the various textures of the lung is an exceedingly common affection, the first question which naturally presents itself seems to be, whether this is, or is not, generally succeeded by the evolution of tubercles ? The answer given by Laennec, Bayle, and others (an answer which appears consonant to general experience) is, that in point of fact the vast majority of cases of pneumonia, are not succeeded by phthisis—whereas, was inflammation of the lung's substance the producing cause of tubercle, this would be frequently if not always the result; that if the opinion be founded on the anterior existence of pneumonia in a *chronic* form, it is untenable on the grounds—that this disease is an exceedingly rare one, while tubercular phthisis is unfortunately common; and that the morbid appearances after death, from the two affections, are altogether distinct.

The old opinion that tubercle is the sequel of catarrh is combated by Laennec with equal decision, and, in my opinion, success.* He admits indeed that true phthisis often developes itself with symptoms similar to catarrh, but in these cases it is equally probable that such are consequent on the irritation of existing tubercle, as that those bodies are the result of inflammatory action; on the other hand, tubercles undoubtedly often exist in the lungs of persons who never remember to have had catarrh at any period; while conversely, of the numbers every year affected with that complaint, very few comparatively become the victims of phthisis.

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* "It is evident that in chronic peripneumony, the inflammatory engorgement is confined to the air cells, which are seen closely pressed together, like the eggs of certain insects, without any intervening space, all of the same colour, and of a reddish, greenish, or yellowish colour. Tubercles, on the contrary, are either diaphenous, or quite transparent, and however numerous they may be, are always disseminated, at least in their earliest stage, through the sound and crepitating lung."—*Forbes's Translation of Laennec, p.* 292-3.

The arguments on the opposite side of the question are chiefly derived from the admittedly frequent co-existence of inflammation and tubercle; but their necessary etiological relation, except in the opinion of the zealous Broussais himself, seems in some measure disproved by the very arguments adduced in its support. Thus Dr. Alison admits that "tubercles may frequently form without being preceded by inflammation of such a character as to be detected by symptoms during life," and that the unequivocal marks of inflammation found after death "have really been posterior to them in date;" but he still believes that "inflammation, acute or chronic, becomes in *certain constitutions* the occasion of the development of tubercle."

These sentiments, and they are apparently similar to the conclusion at which Andral has arrived, appear in themselves in a great measure to invalidate the opinion that inflammation can of *itself* produce tubercles; they indeed show—which is not usually denied—that the two states frequently co-exist, but in concluding that the one condition can produce the other only in "certain constitutions," they leave the main question much where it was found;

for if a certain constitution is required to form, as it were, the substratum on which inflammation acts before tubercle can be produced, it amounts to very nearly the same thing as saying, that its deposition depends on some *other* unexplained condition of the body which is only called into action by the inflammatory process.

Nor do the facts elicited either by inspection of bodies dead of phthisis, or by direct experiment on healthy animals, countenance a belief in the inflammatory origin of tubercle. I will not here enter on the question, whether the small grey bodies constituting the granular variety of Bayle, and which Laennec recognises as tubercles in their early stage of formation, are really so, or the result of some other pathological condition; and merely advert to the crude tubercle of the latter author, where a yellowish color having been assumed renders it no longer a matter of doubt that the disease actually exists. In this state we at least avoid the possible fallacy of assuming as tubercle that which is not so, and consequently the anatomical examination of a lung so situated affords the best evidence on the question immediately under discussion.

If in such an example the lung be carefully examined, its texture often appears perfectly crepitous and healthy to the edge of the tuberculated part: there does not exist any consolidation of the tissues around it, or any appearances by which the anterior presence of inflammation is usually recognised. If, in farther prosecution of the inquiry, we inject the air tubes or pulmonary artery, the matter employed freely pervades these tubes to the edge of the foreign substance, and on a section being made, the appearance is presented of a yellowish mass, of variable size, imbedded like the stone of a seal or ring, in a coloured setting, formed by the injected pulmonary substance. Every portion seems free from disease, with the exception of these points, which constitute the germs of a pathological state, destined indeed in most examples to increase, but which in its earlier stages exhibits no one mark of previous inflammation.

It is true, that as phthisis advances to its close, very different appearances present themselves—adhesion of the pleura lining the chest, to that enveloping the lungs, occurs in the great majority of examples. The portions of lung which intervene between the tubercular masses become not unfrequently consolidated by inflammation; the whole organ in fine, by many lesions, often announces its previous existence; but those lesions appear to me to indicate only one of two things—either, that the two states frequently co-exist in the same organ, or that inflammation is commonly produced by the irritation of the foreign matter, and is consequently to be regarded as its effect, and not its cause.

The advocates, however, of the inflammatory origin of tubercle, not satisfied with the equivocal evidence which morbid anatomy supplies, have appealed to direct experiment. It has been stated, that by simple irritation of the bronchial membrane, or of the air cells, if such exist, by mechanical means, a matter is produced altogether similar to tubercle—productive of the same evils, and tending to the same results. "I injected," says M. Crouvillier, "two ounces of mercury by an opening in the trachea of a dog; the greater part was rejected by cough; he showed symptoms of phthisis, and died in a state of marasmus in a month; the lungs were studded with tubercles, isolated and agglomerated, having all the characters of miliary tubercles."*

Had similar results always attended on similar experiments, they would doubtless go far to prove the direct connexion between bronchial irritation and tubercular deposit, but very different effects have arisen from trials made by other hands. M. M. Andral and Lombard, who repeated them with due precautions, arrive at contrary conclusions. "The mercury," they say, "contained in the small bronchiæ was surrounded by a thick covering of puriform mucus, quite liquid in certain points, and in others, resembling the false membrane of croup when yet semisolid. In many cases the bronchial parietes were torn, and the mercury extravasated into the parenchyma of the lung, being there surrounded by purulent matter; we observed no other changes."*

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* Nouveau Biblioth. Med. 1826, p. 381.

I have tried several experiments, with the object of satisfying myself on the merits of this disputed question. The animals employed, were rabbits; of five operated on, three only survived the immediate evils of the experiment. Those in a few days recovered their usual health; two were killed, and their lungs examined within two months; the mercury in each had descended by its own gravity to the lower edges of the lobes, and appeared to occupy the true air cells of the lung; each minute globule of the metal was surrounded by a thin layer of soft whitish matter, similar to that described by Andral and Lombard; the lung was perfectly healthy and crepitous around, nor was any appearance observed which, except as regards colour, at all resembled tubercle. The third animal I retained for more than fifteen months. He continued to grow and flourish so, that being always well fed, he had attained a size rather unusual for his kind, and was remarkably vigorous and fat; proving that the presence of the foreign matter in his lung had not interfered with the nutritive process, or produced any evil after the immediate shock of the operation had been rallied from.

The lungs of this animal are now before me, and they exhibit the following appearances: the mercury is contained chiefly in the lower edges of the lobes of the left lung, a very small portion only having occupied the right one; externally, this part presents many dark-coloured round elevations, covered by the serous membrane, and feels of a solid texture like liver. On cutting into it, each globule of mercury is found to occupy a small round cavity, lined by a white pulpy substance, thicker than pus, exterior to which the lung is consolidated, so that on removing the metallic globule, a round cavity remains. The consolidation ends at the upper line, where the mercury had ceased to descend, and the organ above this is entirely crepitous and healthy.

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* Precis D'Anat. Pathol. vol. xi. p. 551.

It appears to me from the preceding remarks, that direct experiment, the conclusion of morbid anatomy, and the result of observation in cases of phthisis, equally militate against the belief that inflammation can of itself establish the condition on which that disease depends; that those lesions, obviously the result of inflammation, which so frequently co-exist with tubercle late in the complaint, must be viewed either as examples of the simultaneous occurrence of two different diseases in the same organ; or, what is more probable, of inflammation resulting from the irritation of the tubercles themselves; or finally, that if inflammation ever indeed leads to the deposition, it does so merely by increasing some other unknown variety of morbid action, which is essential to its production; from which it follows, that the nature of *this* action itself is the question of all others most necessary to be attended to, with a view to the advancement of our knowledge on the disease.

We shall yet have occasion to see that the inflammatory or non-inflammatory origin of pulmonary tubercle is by no means a speculative question, but, on the contrary, involves matters of high import as regards treatment; if we answer it in the affirmative, the practical deduction necessarily leads to the employment of those means which, by universal consent, have been applied to subdue an excited condition of the vessels, and this, not with the intention of merely combating symptoms which may arise during the progress of the malady, but as meeting the cause on which this depends. On the other hand, if we arrive at the negative conclusion, the danger may result, of considering the disease as one of true debility, thus inducing us to neglect a due consideration of the inflammatory process by which it is frequently accompanied. Judicious practice demands a strict attention to this distinction; the difference between *acute* and *chronic* phthisis, as they have been named, is probably dependant on the co-existence, in the former case, of a condition produced by the local irritation of tubercle; and although in such examples it is doubtless a leading object of practice to meet the additional evils which the case involves, it still appears to me that the measures directed to this end require always to be modified by the consideration that we have here to deal with an effect, and not a cause.

2. *Is pulmonary tubercle the result of an error in the function of secretion?*

Contrary to the high authority of Laennec, who considered tubercles to be endowed with the attributes of life, a very general conviction has prevailed, that the matter forming them, though obviously the product of organization, does not, after its formation, exhibit those properties by which life is marked; that they possess no circulation, and hence owe their original development and future increase to the deposition, in living parts, of a dead matter foreign to their healthy organization.

The question therefore presents itself, how this foreign matter is produced; and as the function of secretion is that by which not only morbid but healthy products of dissimilar character are usually drawn from the common current of the blood, so a very usual impression has been, that a solution of this obscure question might be found in a morbid action of the secerning vessels.

There are two methods which nature employs in elaborating new substances from blood: most commonly this is accomplished through the intervention of glands, but not unfrequently the minute vessels distributed on membranous surfaces, or in the substance of organs especially under circumstances of disease, perform the same office; and it has been conceived that the matter of tubercle may therefore be produced in one of two ways—either by glandular bodies situate in the lungs, or by their minute arteries alone, independent of any glandular action. It would certainly be difficult to adduce proof that tubercular matter owed its origin to a morbid change in the first of these processes. The only pulmonary glands, so far as anatomy reveals, consist in the mucous follicles distributed liberally over the lining membrane of the organ.* To what extent this membrane penetrates into the substance of the lung, is still unsettled, but we at least know that it covers the inner surface of the larger air tubes; and provided the opinion be correct that the mucous glands there placed form the seat of tubercle, we should be justified in expecting that they would usually occur in situations where the membrane is most extensively distributed. The facts with which morbid anatomy supplies us, appear fatal to the hypothesis; very rarely, if ever, do we find tubercles placed in such situations, but constantly in the substance of the lung, associated with the extreme divisions of the air tube, or if connected with its larger branches, placed exterior to the cylinder, and consequently not situate under its lining membrane.

As then it appears that pulmonary tubercles are rarely, if ever, found in the only tissue of a compound organ where glands demonstrately exist, we seem borne out by negative proof in concluding that they do not owe their origin to any error of glandular secretion.

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* I here take no account of the bronchial glands, chiefly placed at the lower end of the wind pipe, which, though frequently participating in the disease, as regards *their own substance* can have nothing to do with the deposit in other portions of the lung.

But the various tissue of the lungs are largely supplied with exhalent arteries, and an old idea has prevailed, that tubercle was a deposit, resulting from some change in the perspiratory secretion. This opinion was held by Bennet, one of our earliest writers on the subject, and when to this we add that it was also that of Sydenham, it is one not to be lightly regarded.* In our own day, when the labour bestowed on morbid anatomy enables authors to speak with more precision on such subjects, the same opinion is not without its advocates, among whom Andral may be named as holding a prominent place. "As," says he, "tubercle is produced by the *perspiratory secretion,* of which it appears to be a morbid alteration, it may be developed in any part of the body, the situation however in which it is most usually secreted seems to me to be the cellular tissue, whether free or uncombined."†

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* "Moreover as the blood in winter abounds with moist particles, and perspiration is too much checked by the sudden contraction of the pores, these particles insinuate themselves into the lungs, through the ramifications of the arterial vein or pulmonary artery, which runs through the whole substance of the lungs."—*Sydenham's Processus Integri., Swan's Translation, p.* 664.

† Andral's Pathological Anatomy, Translated by Drs. Townsend and West, Vol. 1, p. 518.

Did it not appear almost presumptuous to question the opinion of so distinguished a pathologist, one might be disposed to say that the results of experience, as reported in his own work, go far to invalidate the conclusion here advanced. If tubercles really depend on an error in the perspiratory secretion, we are justified in assuming that they would be most frequently found in parts where this function is carried on with the greatest vigour: of all tissues of the body this is, without doubt, that of the skin, and yet it is a curious fact, that the tables contained in the work of this author, deduced from a large experience, and attested by authorities, among whom may be enumerated M. M. Louis, Lombard, and himself,* do not present us with one example of tubercle located in the very tissue where, according to his own hypothesis, they ought most frequently and certainly to occur. It is scarcely necessary however to appeal to such high authority for proof of the fact, that true tubercular deposit is seldom found in the cutaneous textures. The same scrofulous constitution indeed with which tubercle is usually connected, often from weak or obstructed circulation induces that unhealthy form of ulceration which we see developed on the surface, but whatever be the relation in which this stands to tubercle itself, it seldom if ever commences there in the same manner, or runs the same course.

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* Oper. Citat. Vol. 1, p. 521-526.

But the function of perspiration, it may fairly be said, is not peculiar to the skin: one quite analogous occurs on all the cellular membranes, "combined" as well as "free,"* and hence, as we know that this enters into the intimate contexture of glands, of muscles, and other parts where tubercles very generally have their seat, the fact may still be that those depend on some error in the perspiratory process. The most valid answer to this objection perhaps may be found in a recurrence to the argument before employed. If an abnormal deposition be referred to an error in some special function, we are entitled to demand direct evidence of the truth of the opinion, and provided such is not afforded, a strong counter-argument presents itself in the fact, that the effects of this error are *most* frequently observed in those very situations in which the healthy function is *least* exercised.

On such grounds do I venture to offer an opinion, that the evolution of tubercles is not explicable on the hypothesis, that they owe their existence to an abnormal condition of the perspiratory function.

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* The terms "free" and "combined" refer to the same tissue, and consequently imply identity of function. The inappreciable cellular membrane, which binds together the all but ideal *ultimate* fibre of a muscle, is physiologically the same as that which connects the skin with subjacent parts.

3. *Does tubercle result from an error in the function of nutrition?*

It must ever remain a subject of regret, that the extraordinary talents of Laennec had not been applied to a greater extent in investigating the nature, seat, and mode of production, of tubercles of the lung. The great debt of gratitude which humanity owes him for his admirable description of their physical characters after formation, and the still greater debt, for the suggestion of means calculated to detect their presence in the living body, would doubtless have been increased, had his attention been also directed to the nature of the vital changes which precede and accompany their evolution. On this part of the subject his treatise is certainly defective, and communicates but a slender store of information. It is true indeed that Laennec expresses his conviction, that tubercles are produced by *some* error in the nutritive function, and that on his high authority, such opinion is still held by many members of the profession : but assuming this to be the correct one, there is here certainly something more demanded than the simple expression of opinion ; we desire to know, if possible, not only the particular function involved in the production of tubercle, but the special character of the morbid change which this function sustains ; for, although it be undoubtedly true that a marked impediment to the process of nutrition attends on every case of phthisis, it is not the less true that this suffers in nearly an equal degree where no tubercular deposit exists, and hence, before an error in the nutritive function can with certainty be assumed as its cause, it behoves us to inquire into the nature of this error, and how it acts in the production of the disease. Besides this, it would be desirable to ascertain whether the impediment to nutrition which attends on phthisis is to be viewed as the cause of tubercles, or as the effect; whether these bodies are produced as a consequence of some derangement in the nutritive function common to the lungs and other parts; or whether those others are effected as a result of pre-existing tubercles, originally produced by some other cause, remotely or immediately acting on the lungs alone. These points involve matter of serious consideration, suggested undoubtedly by the opinion of Laennec, but as certainly not determined by his writings.

In taking cognizance of this question, we must not forget the peculiar relation in which the lungs stand to nutrition. In the first place they, as organs, derive their nourishment from blood; and in the second, they prepare and render fit the whole mass of blood for the execution of this function; for however obscure the nutritive process may be, we at least know with tolerable certainty that the products of the earlier stages of digestion require to be identified with blood by the action of the lungs, before they are in a fit state for final deposition. Here then these organs must be regarded as *remotely* necessary to the nutritive process; but when the lungs have identified chyle with blood—or, as the term goes, sanguified the products of digestion—they have still as regards the act of nutrition another important duty to perform, simultaneously with the first. The blood may be perfectly charged with new nutriment, and yet this is unavailing, unless the lungs duly execute the act by which venous blood is changed into arterial, conferring that temporary quality which enables the capillaries of organs to apply this fluid, in itself properly constituted, to their wants. These organs, therefore, as the executors of the respiratory function, effect, as regards nutrition, two conjoint purposes—the one *immediate* and the other *remote*, and are hence self-sustaining organs, producing the blood by which they are themselves nourished.

To effect such ends, two distinct sets of arteries pervade their substance; one set are passive agents, convey black blood, and are simply destined to place this fluid under conditions favorable to the reception of its vital character. This, I need hardly add, includes the pulmonary artery and its ramifications.

Another set of arteries, deriving their origin from the left side of the heart, much smaller in number but active in function, convey red blood, and supply them with the fluid which supports their own vitality. I here of course allude to the bronchial arteries. By the *active* agency of one set, the lung is retained, as an organ, in a condition fitted for the execution of its important functions; by the *passive* agency of the other, it is supplied with the fluid towards which this function is directed.

When therefore, in accordance with Laennec's opinion, we attribute tubercle of the lung to an error in nutrition, it appears reasonable to surmise that this may arise in one of two ways :—either it may depend on some erroneous action of the vessels which nourish the lung, or on some aberration of the vessels supplying the fluid on which as an organ it acts.

In either case the result may be presumed to be similar, as regards the general function of nutrition, inasmuch as it amounts to the same thing whether this suffers in consequence of some defect in an organ whose integrity is necessary to its due execution, or whether in consequence of an impediment to the transmission through it of a fluid towards which such function is directed.

But the question assumes a different aspect when it is brought to bear on the actual origin of tubercle, and it is here that the opinion advanced by Laennec requires extension. He may be perfectly correct in attributing tubercle of the lungs, in general terms, to an error in nutrition, but he leaves us entirely uninformed, not only as to how this error is remotely produced, but also as to the immediate mode in which it determines the deposit.

As some portion of the future pages of this volume are applied to the attempted elucidation of this question, I shall in this stage content myself with saying, that of the three distinct opinions now noticed, that of Laennec appears to approach most near to the truth.

The remarks now offered seem to warrant the conclusion, that we are yet in a great measure uninformed on many important points in the pathology of phthisis. It would appear that the abnormal action of the vessels, which we express by the term inflammation, is in no way adequate to account for the development of the material cause of the disease; that the opinion which attributes its production to an erroneous action of secretory arteries, is not only destitute of positive proof, but in itself improbable; and that the one which refers it to some error in the function of nutrition, though true to the extent that this function always does suffer during the progress of the malady, still leaves us quite in doubt, not only as to the nature of this error, but whether it is to be considered as the cause or effect of the deposit, and consequently, while it conveys no distinct information as to the origin of tubercle, still less affords any data from which a rational practice in the disease may be deduced.

The uncertainty, therefore, in which the pathology of phthisis remains, renders it a subject still legitimately open to further inquiry, and on these grounds has the author ventured, after some reflections on the opinions of others, and some practical knowledge derived from his own observation, to submit the results of his experience on a question, whose successful elucidation has certainly not been commensurate with the talent and labour hitherto bestowed on it.

There is every reason to believe that the morbid phenomena exhibited in cases of consumption are of a compound character, and own distinct origins, which equally demand the attention of the physician, whose aim is directed not merely to the alleviation, but to the cure of the disease. All agree that the location of tubercular matter in the lung depends on some constitutional cause existing anterior to the deposit, and of which it is the result; this may be properly viewed as constituting the first step in that complex morbid process which the general disease involves, and may be properly designated as *an abnormal action of the system towards the lung.* But it is equally certain that, when this has produced its effect, another series of symptoms arise, which owe their origin to an imperfection in organs important to the well being of the whole body, which may hence be characterized as resulting from a morbid *re-action of the lung towards the system;* and though it be true that the progressive nature of phthisis is such, as to render it probable that this action and re-action alternate with each other, even to the termination of the disease, yet the distinction between them seems to me sufficiently clear to warrant its adoption as a basis of arrangement.

But if this offers a groundwork of arrangement as regards the pathology of phthisis, it follows as a necessary consequence, that remarks directed to its treatment will naturally arrange themselves also under corresponding heads—the first, relating to the means calculated to counteract a tendency to deposit tubercle—the material cause of the disease : the second, such as bear on those, proper to avert the constitutional evils which are induced by this cause after its establishment ;—and in either case we have to hold steadily in remembrance, the connexion which exists between those two phases of the malady, ever considering that the action of the system towards the lungs, and the re-action of the lungs towards the system, reciprocally tend to influence each other.

In conformity with the views above expressed, I propose to divide the succeeding portion of this work into certain chapters, which may be sufficient to embrace, with tolerable perspicuity, all I have to say on the subject; and to conclude by a report of cases, in illustration of the practical efficiency of those principles which it has been the object of its previous pages to explain.

CHAPTER II.

TUBERCLES OF THE LUNG—THEIR REMOTE DEPENDANCE ON CONSTITUTIONAL CAUSES—THEIR IMMEDIATE DEPENDANCE ON LOCAL ERRORS OF ACTION—THEIR EARLY APPEARANCE—GRANULATIONS—CRUDE YELLOW TUBERCLE—THEIR NON-IDENTITY MAINTAINED—COMPOUND STRUCTURE OF TUBERCLES—APPEARANCES ON INJECTING PULMONARY CIRCULATION—THEORY OF THEIR LOCAL FORMATION—OF PREDISPOSITION AS CONNECTED WITH ORGANIZATION—SPECULATIONS AS TO THEIR REMOTE CAUSE—CONCLUSIONS.

In accordance with the arrangement adopted, we have in the first place to inquire into the nature of that action, originating apparently in the system, and directed towards the lung, of which the deposit of tubercles in the latter organ is the result. Two important points are necessarily involved in such inquiry. First, the nature and character of the primary constitutional disturbance which tends to the production of the local affection. And secondly, the mode in which this actually leads to its establishment.

The first, clearly involves a consideration of what has by the older writers been termed the phthisical diathesis, or in modern times been treated of by Sir James Clarke and others, under the more novel name of tubercular cahexia.

The second, with equal propriety, may embrace a consideration of the physical character of tubercles—the probable mode of their formation—and the tissue which they occupy. Our acquaintance with the first, must be derived in a large degree from observation of symptoms; for as happens in other functional diseases, anatomy throws little if any light on the nature of those vital changes which precede the evolution of tubercle; and though it may possibly appear in the sequel, that what has been termed the strumous diathesis, is more or less susceptible of connexion with peculiarities in the structure of organs; this is after all a matter of inference from the information which the scalpel furnishes after organic changes have arisen.

The case is however very different in regard to tubercles when formed; these bodies having become objects of the senses, can be submitted to close examination; the legitimate and only useful end of such examination being, not merely to attain a knowledge of their site, form, and other physical properties; but from an acquaintance with such properties to deduce the nature of the morbid action by which they were originally produced. In this view the investigations of morbid anatomy, here, as in other instances, are to be considered as the means only to an end; they point out the organic lesions which succeed functional change, and thus afford data, which reason may properly employ in inferring the nature of those vital errors by which they were preceded.

In strict propriety of arrangement, I should here perhaps first advert to the peculiarities of structure, as well as action, which are connected with the tuberculous diathesis; but, independent of the fact that the symptoms of this state have been already minutely and well described by several modern authors, I venture further to offer an opinion, that any satisfactory solution of the mode in which this diathesis acts in producing the local disease, must be deduced from examining the deposition which appears as its effect; endeavouring from this to infer a probable explanation of the mode in which it occurs, and, as a matter intimately connected with this, the texture which it occupies—thus leading us one step backward to the primary aberration from healthy action, which, though perhaps ever destined to remain obscure, must still be considered as the proximate cause of the disease.

On these grounds I propose in the first place to offer such observations on the nature and site of pulmonary tubercle, as have occurred to me, after some years of tolerably assiduous application; and to reserve any remarks on the phthisical diathesis, which may appear necessary, to follow afterwards; thus ascending as it were from effect, to cause, instead of following the reverse order of succession, which, without a due consideration of the difficulties connected with this still unsettled question, might certainly appear to be a method more correct.

A serious difficulty presents itself on the very threshold of the inquiry now proposed. The difficulty consists in this—what physical characters do tubercles of the lung present in their earliest stage? by what marks are they at that period so distinguished, as to enable us to conclude that they are actually present?

It will at once be perceived by every pathologist, that I here refer to the question still agitated, in regard to the identity of pulmonary granulations, and crude yellow tubercle.

By the first term, we express, small roundish semi-transparent bodies, usually cohering together like a bunch of grapes; and generally connected with a bronchial tube. By the second, certain yellowish white points, sometimes associated intimately, sometimes disunited, of variable size, cheesy consistence, composed principally of albumen, and tending after a certain period, to pass into a dissolved or softened state. Between the physical characters presented by these two sets of abnormal growths, there thus exists a marked distinction; and it may be here observed, that the genuine yellow tubercle is often found of a magnitude not at all exceeding that of the semi-transparent grey body, which is by some assumed to be its germ.

Bayle and Laennec, the two earliest investigators of this point in modern times, are entirely at issue on the question. The former considers granulations of the lung, as constituting a variety of morbid growth, *sui generis;* and consequently as distinct from tubercle, although they form the material cause of symptoms exceedingly analogous to those resulting from the presence of the latter. The "granular," therefore, in his arrangement, forms one species of the general malady, which, as already remarked, he treats of as a genus, under the name of phthisis: granulations, on this view, have no other connexion with tubercles, except that they originate similar symptoms.

His successor Laennec, on the other hand, considers the former bodies to constitute the earliest appearance which tubercles present,—to be consequently necessary to their existence,—and hence as productions, not only frequently associated with the tuberculous state, but essentially and fundamentally connected with it under all circumstances*. It must certainly be conceded that the grey granulation, and tubercle unequivocally announced by its physical characters, very generally co-exist in the same lung, so generally as at first sight to render the opinion of Laennec highly probable. Of 358 cases which came under the observation of M. Louis, two only presented yellow tubercle without granulations, and five only contained granulations without tubercle: nor do I conceive that the results furnished by the experience of less practised inquirers will differ much from the more precise estimate of this pathologist.

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* "Bayle," says he, "was evidently mistaken in considering these granulations as different from tubercles."—*Laennec on Diseases of the Chest.—Translated by Forbes, p.* 274.

The frequent co-existence however, of the two affections, no more establishes their identity, than do the frequent evidences of inflammatory action in phthisical lungs prove the dependance of tubercle on that condition; and there appears strong grounds for the belief that, while granulations are generally the result of chronic pneumonia, tubercles depend on some altogether different condition, which, by establishing a continued irritation in the organ, leads to that low description of inflammation from which the former spring. On this supposition tubercles are, at least occasionally, the cause which induces granulation; but it seems more than probable, that in this respect the two states act and re-act, and that in some instances irritation of the lung otherwise established, may predispose the organ to take on a tubercular action;* so that without adhering to the opinion of Laennec as regards the *identity* of the two affections, we readily perceive a reason for their being very commonly associated in the same subject.

The identity of tubercle and granulation is combated by Andral with considerable success. "If tubercle," says he, "commenced necessarily by being a greyish semi-transparent granule, this latter should have been observed in every part of the system where tubercles have been found. Now I would ask, have such granules been observed in the lymphatic ganglions, where tubercle can be followed in all the phases of its development? Have they been found in the brain, in the liver, in the spleen, in the submucous, subserous, or intermuscular cellular tissue?"†

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* Tubercle has an especial tendency to develop itself where there exists a chronic process of irritation.—*Anatomical Pathological Translation.—Vol.* 1, *p.* 511.

It is however true that these arguments lose all their force, provided the statement of Laennec be in accordance with the fact, that granules similar to those of the lung "develop themselves in different systems of organs;" for they are entirely founded on the assumption that this position is incorrect, and considering the great talents and large experience of the author whose sentiments are here impugned by his successor—and what perhaps ought to carry greater weight, the high integrity of purpose which pervades his writings, we are not justified in lightly disregarding an opinion which emanates from such a source.

When Laennec asserts, in corroboration of his general proposition, that "The development of tubercles in other organs presents also a series of facts sufficient to prove that in their first state, and near their origin, they are always *diaphanous or semi-transparent*, or *colourless*, or of a *slight grey colour*," he is certainly so far borne out by facts, that small bodies very similar to granulations of the lung frequently exist in organs which are often the seat of true tubercular deposit, especially the serous and mucous membranes; but it is here to be remarked, that strong analogy no more establishes identity between these and granulations of the lung, than it does between those latter bodies and the pulmonary tubercle of which they are by him assumed to form the basis.

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† Ap. Citat. vol. 1. p. 509-10.

Such is the view apparently taken both by Chomel and Andral: the former, in broad terms asserts that they have nothing in common but the name; while the latter, having before offered his opinion that granules of the lung result from hypertrophy of its air vesicles by chronic inflammation, extends this to other organs; conceiving that the analogous bodies which there appear, and are identified by Laennec with them, as equally constituting the elementary nucleus of tubercle, are in truth either enlarged follicles of the mucous coat in the one case, or minute specks of incipient false membrane in the other.

According to this opinion, tubercles and granulations have no more necessary connexion in the organs of the system, than in the lungs; but in both situations the latter are simply the result of chronic irritation, its products being slightly modified by the nature of the tissue in which they are evolved.

To the arguments thus founded on anatomical investigation, in the validity of which my own more limited experience induces me fully to concur, I would desire to add another, derived from the action of re-agents in the first place, and the simple process of decomposition in the second, on lungs which present the two varieties of morbid growth, already noticed.

When slices of healthy lung are placed in water for a length of time, and the bottle frequently agitated, they ultimately form, by the process of natural decomposition, a dark slimy solution, with fibres or threads diffused through it—the coats of vessels, or bronchial tubes, which have withstood the putrifactive action longer than the true parenchyma of the organ.

When the lung so treated is occupied by the semi-transparent granules only, and free from crude yellow tubercle, the same results are obtained—the granules decompose with a facility equal to the natural tissues, and become, in common with them, diffused throughout the liquid: but when again the portion of lung employed contains yellow tubercles, which have not as yet passed into their softened stage, the texture of the lung, though as in the other cases resolved into a thick solution, contains within it numerous whitish yellow masses, of variable magnitude, readily diffusible by agitation, but which exhibit a strong tendency to become precipitated.

These are tubercles, originally contained in the tissue of the organ, which, resisting the putrifactive process longer than the textures which they occupied, become freed from their attachments, and in accordance with their relative gravity, sink usually to the bottom of the vessel. By now pouring off the supernatant fluid, and repeating the process by fresh ablutions many times, the tubercles may be obtained, by a little management, nearly free from pulmonary substance, and retain their colour, shape and consistence, even in water, but still better in diluted alcohol, for a considerable time.* The results thus obtained by the long continued action of water on lungs containing the two varieties of deposit, appear to strengthen the arguments before adduced, as bearing on the non-identity of pulmonary granulation and true tubercle; but though certainly confirmatory, they are not conclusive; for it may be argued fairly, that the matter forming those bodies might be still the same, and yet in its very

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* I have now in my possession several bottles containing tubercles procured in this way several years ago: some of those are in water, and have undergone no change except as regards colour; they, and the fluid in which they swim, becoming of a brown hue, especially in hot weather: in diluted spirit this does not happen.

I have said in the text that they usually precipitate, but to this there are a few exceptions, the tubercle when detached from its adhesions tending to the surface of the liquid. This difference might probably be traced to the relative quantities of animal matter and earthy salts which various tubercles contain. According to the analysis of Therard, while one hundred parts of unsoftened tubercle taken from one subject contained

Animal Matter	98 15	= 100
Earthy Matter	1 58	

The same quantity taken from another body, where they had undergone a sort of cretaceous change, contained inverse proportions of these constituents, or

Animal Matter	4	= 100
Earthy Matter	96	

Between these extremes we may fairly presume, that the proportions vary almost infinitely in different cases.

early stage too soft to retain its cohesion under that long maceration which, when fully consolidated, it became capable of resisting. Admitting therefore the possibility that such mechanical difference may have led to an uncertain conclusion, we might at least fairly expect, that an identity of chemical constitution would be attended by an identity of chemical relation; and hence reasonably conclude, that if certain re-agents act differently on the two varieties of deposit, the fact would go far to confirm the proofs of their distinctive character otherwise attainable. My original object in endeavouring to obtain unsoftened tubercles free from the pulmonary textures, was, with the view of submitting them in their detached state to the action of different solvents, hoping that some practical deduction might thus be made in regard to their solubility in the living body.

Among different substances employed for this purpose, whose action in a future chapter I shall detail more at large, solutions of the pure fixed alkalies, were those re-agents alone which I found to act with any energy on the matter employed.

When a slice of lung, containing yellow tubercle in its unsoftened state, is placed in pure liquor of potash, its texture, though after some time it becomes somewhat gelatinous and soft, is otherwise but little acted on, while the tubercular matter it contains, wherever by the section it is exposed to the free action of the solvent, speedily disappears.

When tubercle freed from pulmonary substance is treated in the same manner, a clear saponaceous solution results, only rendered a little turbid by such few shreads of the lung's texture as may still have adhered to, or been imbedded in the detached tubercle. When again, a portion of granulated lung is subjected to a similar process, no solution occurs; the textures of the organ as well as the granules are indeed softened and somewhat swelled by the action of the alkali, but in other respects retain their original appearance; and the clusters of granulation, when examined afterwards by the aid of a glass, present the same grape-like appearance which they did in their primitive condition. We appear in these experiments to possess distinct evidence, that the same substance which acts energetically on the matter of tubercle, in the earliest stage at which its presence can be indisputably admitted, acts but feebly on those granular bodies which have been assumed as presenting it in a still more early stage of its development; and as it would appear that the advocates of this opinion must necessarily hold the chemical identity of the two substances, though different in some of their physical properties, they ought also I apprehend to admit the strong probability, that they should present an identity also in the chemical relations which they bear to other substances.

The conclusion deducible from these premises is, I conceive, inevitably hostile to the opinion of Laennec, and strongly corroborates the arguments derived from other sources, and by other persons, in support of the *non-identity* of granulation and tubercle, the converse of which position that author has maintained.

I have dwelt at some length on this disputed question, because it appears to me in the highest degree desirable to ascertain with certainty on what marks we can rely, as announcing the earliest appearance of pulmonary tubercle: my conviction has ever been, that if morbid anatomy is destined to be of actual utility in leading us to a successful line of practice in the disease which it originates, this can only be derived from its supplying us with grounds of rational inference as to the original cause, seat, and nature, of this morbid production; and it is not in the advanced stages of the complaint, when the lung is occupied by excavations, and its textures altered in their physical characters by the effects of inflammation, that the grounds of such inferences can be best obtained. On the other hand, by observing this morbid deposit in the early stage of its formation—reasoning back from this, to some probable physiological principle capable of explaining the *action* by which it is produced—and endeavouring to confirm the correctness of such inference by direct experiment—we may perhaps yet hope to arrive at practical conclusions not altogether destitute of value.

For the reasons set forth in the preceding pages, I shall assume that the grey semi-transparent granulations have no necessary connexion with pulmonary tubercle, but that this latter substance first shows itself in the lung as a yellowish friable matter, in points of various magnitude, of a round or ovoid form—that these points in their earliest stage are usually, if not always, exceedingly minute, —and that they apparently increase in magnitude, not in consequence of any inherent capacity of enlargement in themselves, but from the approximation of many, fundamentally distinct; the variable size of the tubercle (as so called*) being thus dependant on the number of smaller points which it contains.

I shall yet have occasion to refer more particularly to the appearances which would seem to demonstrate the really compound structure, even of the most minute tubercle which the unaided eye can recognise, and certainly of those of greater magnitude; I here merely remark that the latter, when of sufficient size to admit of a section of them being made, usually attest the truth of this observation, by the thin partitions of pulmonary substance which intervene between the particles, or minute points, of which they appear to be composed.

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* The word tubercle is, in strictness, merely the diminutive of tuber—a term employed, from the days of Galen, to indicate any swelling of a roundish form, without regard to its nature or composition. The distinction depends on magnitude, and it would be therefore difficult to say in such sense where any swelling ceases to be a tubercle, and becomes a tuber (or tumour). The word is now however adopted as a conventional term, to express a small swelling caused by a morbid matter of peculiar character, and in such sense I invariably use it.

Similar varieties of opinion to those which I have already noticed as dividing pathologists on the action by which tubercle is produced, equally divides them on the question which relates to the special texture of the lung it occupies after formation; indeed, the one point is almost unavoidably connected with the other; for, as the lung is composed of several tissues differing in anatomical structure as well as vital attributes, it follows as a necessary consequence, that when we ascribe the formation of tubercle to the aberration of a particular function, we must also presume it to occupy the tissue of which such function is the seat; thus, if tubercle depend in truth on an erroneous secretion, it seems clear that we should expect to find it on that free surface where the bronchial glands or garglions pour forth their natural products: if again, we consider it as the result of a change in the nutrition of the organ, we might equally infer, that as this function proceeds in all its tissues, so tubercle should occasionally occupy them all—or if again, inflammation be assumed as its cause, the same argument will apply with even greater force, for every texture of the lung is undoubtedly susceptible of sustaining the structural changes induced by that abnormal action of the vessels which is comprehended in the term. Laennec has not himself entertained the question of tubercular locality, though his opinion of the cause producing this, would lead us to infer that he believed them susceptible of development in any tissue, but more especially the cellular. His cousin, M. M. Laennec, who adheres generally to his relative's views, considers them to be deposited in the intimate contexture of organs, where all the processes of nutrition, whether normal or abnormal, go on; and in this he is supported by many contemporary pathologists. Others again have considered the air cells* to constitute their usual seat—an opinion held by Andral in France, and by Dr. Carswell in this country; while many who partly adhere to such belief consider that the same action which leads to their deposition there, may also produce it in the common cellular tissue of the organ.

To the probability of the opinion, that tubercle occupies the air cells, a serious objection has been stated by Dr. Lombard of Geneva, that as these cavities communicate freely with the trachea, it is inconceivable how, under such circumstances, their contents should not become occasionally detached and extruded in their early stages, a thing well known never to take place till they have passed into a softened state. This objection seems to me considerably strengthened by the tenacity with which the matter of tubercle adheres to the lung when first formed. If the white cheesy substance was simply an extraneous deposit, thrown out by certain exhalent vessels into the minute cells or cavities either of the breathing part or common cellular texture of the lung, we might reasonably expect that it would be susceptible of easy detachment from the parts containing it; and yet, any person who has examined with care even a single one of these bodies in its crude state, must have observed how strongly they adhere to the parts around them, as if each particle was bound down and retained by some connecting membrane, a fact which militates strongly against the supposition, that they result simply from the extrusion of an unnatural product into a healthy cavity. Notwithstanding those objections, the form of tubercles, and many other circumstances connected with their history, appear to me strongly to support the conclusion that they are, in point of fact, usually, if not always, associated with the true breathing portion of the lung, and the question therefore arises, whether there remains any means of reconciling this opinion with the obvious difficulties to which it is liable.

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* In using the term "air cell," it must be considered merely to express that portion of the lung's spongy structure into which the air is finally conveyed, and where in consequence the action between it and blood proceeds. The question is still undecided among anatomists, whether this is to be viewed as a distinct tissue in which the bronchial tubes terminate, or as the mere termination of these tubes themselves. The dispute, after all, is one very much of words. One point seems to me readily demonstrable, by examining the dried section of a lung which has been injected with mercury through its air canals, namely, that their final extremities are of a round form.

Dr. Lombard, to whom we are indebted for suggesting the objection recently stated, appears in some measure to have reconciled these difficulties by concluding, that tubercle is connected with a morbid condition of vessels which ramify on the lining membrane of the air cells; and I cannot help thinking, that not only has he here thrown out an idea of great importance in itself, but the truth of which is also largely confirmed by the observation of other pathologists, however they may have failed to deduce correct conclusions from their own experience. Assuming, however, the general correctness of such a supposition, it still remains to investigate the nature of this morbid state—the causes of its production—and the inferences which it affords as explanatory of the phenomena of phthisis. These are points still open to further inquiry, and to such I now propose directing the attention of the reader.

The minute anatomy of the lung is yet enveloped in so much obscurity, that scarcely two observers agree in the descriptions they have offered. I would not therefore bewilder myself by entering on discussions which have hitherto proved extremely unproductive, but we may assume, with tolerable certainty, that the air which enters by the wind pipe finally arrives at certain cells or cavities, where it is sub-divided with great minuteness, and that the membrane which coats the interior of these is intimately pervaded by vessels still more minute, which derive their contents from the right cavities of the heart. By means of this arrangement, air and blood are brought into close proximity, and that mutual action between them established, on the due accomplishment of which the efficiency of respiration depends. According to this view of the process which goes on in the minute contexture of the lung, it is obvious that a delicate membrane intervenes between the air which is brought into contact with the external surface of each vessel, and the blood which is contained within it, and it is therefore extremely probable, though not demonstrable, that their mutual action takes place independent of direct or immediate intermixture. The capillary vessels, which thus pervade the membrane of the true breathing portion of the lung, differ from those of the aortic system in the nature of the fluid they convey: while the latter carry a blood which is fully perfected in composition, the former transmit a current which not only differs from the other in vital properties and colour, but also by holding in a state of loose intermixture certain fluids conveyed to it from the lacteal and general absorbent vessels. The blood, therefore, which pervades the pulmonary circulation, must be considered as a fluid which contains portions not as yet fully identified with its composition; and a probable conjecture might hence be hazarded on these grounds alone, that as many diseases obviously depend on an erroneous action in the vessels of the aortic system, so some might also result from an imperfect execution, by the pulmonic arteries, of the action by which sanguification is finally accomplished. I hope yet to show that what is here assumed as a rational conjecture, is in point of fact rendered probable, as regards tubercle, by several circumstances; all tending to show, that what has generally been considered as a matter secreted, into one or other of the textures of the lung, is in reality a substance contained originally in the blood, which becomes permanently retarded in the pulmonic capillaries, during the passage of this fluid through them, and that tubercle, in its early stage, is essentially constituted by many of those obstructed vessels applied closely to each other, thus forming a whitish yellow mass, of variable magnitude, according to the number of those which are involved in its formation.

When we employ the term, tubercle of the lung, it must be remembered that we allude to bodies not definable by reference to their size; we express by the term, certain substances foreign to the healthy organization, which, while they agree in colour, and very nearly in composition, vary considerably in magnitude; and we are by no means justified in assuming that the smallest of these growths cognizable by unaided vision, by any means constitutes the earliest stage of their formation: on the contrary, whatever hypothesis we may adopt to explain the cause of their production, it is more reasonable to suppose that this operates and produces its effect at a point anterior to that at which such becomes apparent; and we are therefore entitled to infer the existence of a stage of local abnormal action, preceding that at which its product is generally detected. "If," says Andral, "we infiltrate and dry a lung containing tubercles, the parenchymatous tissue around them being apparently healthy, and after drying, slice it into layers, we observe certain air cells whose cavity is dilated, and whose parieties are considerably thickened, and present a *peculiar yellow tinge;* in some points this thickening becomes more considerable, and the yellow tinge deeper; and in others we are enabled to distinguish in these thickened parieties a number of *minute yellow round bodies which are evidently tubercles.* Here then we find certain lesions precede the secretion of tubercles."*

The lesion which Andral here cautiously consider as one "preceding" the secretion of tubercles, appears to be nothing more than the earliest result of the identical action by which they are produced, and each minute particle of the matter which communicates the yellow tinge he so truly describes, seems the same with that, which existing in larger quantities, no longer leaves the existence of tubercle in doubt. The matter which tinges the membrane is on this view tuberculous matter, produced by the same action and tending to the same end; the only difference being, that in this the earliest stage of its existence, it has not accumulated in sufficient quantity at the points observed, either to obstruct the air cells, or form the comparatively larger nodule which even a minute tubercle presents. I have many times subjected to examination, in the manner described by Andral, portions of lung taken from persons who have died of phthisis, and can with great confidence attest the correctness of his observations.

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* Andral's Path. Anat. translated, v. 2, p. 551-2.

The results of the experiment are most manifest when the part is derived from a subject rather advanced in life, and who has long laboured under consumption of a chronic kind, because in such cases the air vesicles of those portions not actually occupied by tubercles are often in an enlarged or emphysematous state, and as a consequence, permit us to examine their lining membrane with greater facility; but in so far as my experience goes, the same appearances are presented by certain portions of the seemingly unoccupied lung of all phthisical persons, when they are carefully examined. Under favourable circumstances the thickening and general yellow colour of the membrane is exceedingly distinct, and when it is not entirely dry I could often perceive, by the aid of a moderately strong glass, lines of yellowish vessels pervading it, those being most distinct where the general tinge of the cell was least marked, and least so where this was greatest, as if the close interlacement of many minute vessels in the same state prevented any isolated one from being distinguished. The yellow matter which thus appears at first sight to coat the inner membrane of its vesicular structure, in the relatively healthy portions of a phthisical lung, is with difficulty detached, but adheres with great tenacity to the membrane occupied. My conviction is, that this is not the result merely of a cohesive quality in the matter itself, but depends on this being contained within the calibre of vessels, whose coats are intimately associated with the membrane they pervade, if indeed it be not entirely formed by their interlacement; for besides that in the damp state the yellow matter cannot be washed or rubbed away without carrying the membrane along with it, any attempts in the dry condition to effect their separation raises up numerous filaments, closely associated with the matter itself, and in which it appears to have been imbedded.

These appearances render it a probable surmise, that the yellow matter deposited in the membrane of the air cells under the circumstances already named, really constitutes the early germ of a deposition which, if life had continued, would have gone on to establish tubercle there in a distinct form; besides this, they appear to show that the coating thus alluded to is not merely an extraneous substance varnishing the subjacent parts, but one which is bound down and retained on their surface by an interlacement of delicate membranous threads, distinct from itself: nor can I explain this in any other manner, than by attributing it to the retention within the cylinder of minute vessels, of a substance which is usually believed to be already extruded from the circulation into some one or other of those tissues which form the lung.

So far then does it appear probable, that what is generally considered as a tubercle of the smallest magnitude, is after all, but the more extended result of an action which commenced by forming bodies of the same nature, but much inferior size, and which, on account of their minuteness, are very generally overlooked. We have still therefore to consider tubercle, properly so called, in the next step of the ascending scale of magnitude, where it assumes a size which places it appreciably before the unaided eye, and see how far the appearances then observed go towards a confirmation of the preceding remarks.

If we examine a minute tubercle, or cluster of tubercles, with the unaided eye, we perceive in them nothing more than small, generally round, whitish bodies, of seemingly homogeneous consistence, which adhere with great tenacity to the pulmonary substance immediately surrounding them: if these bodies be a little separated from each other, and that no consolidation of the lung exists, either as a consequence of conjestion, or inflammatory action, the intervening pulmonary substance preserves its natural aspect; and if an isolated tubercle be raised by the forceps, it drags after it the yielding and elongated lung, which again reacts on the removal of the extending power, without any separation taking place between it and the morbid growth it contains, unless the force employed has been considerable.

As the centres of deposit become more numerous, so do the intervening portions of lung disappear; the points in consequence approximate each other and coalesce, or appear to coalesce, into masses of greater and greater magnitude, so that before their second or softening stage has commenced, they often become as large or even larger than a well sized almond.

That what might here be considered as a growth or increase, arising from causes inherent in the tubercle itself, really depends on the coalition of many bodies of the kind, seems proved by an examination of the interior structure of any considerable mass to which we apply the name; and indeed it appears to me probable, though certainly difficult of demonstration, that the very most minute of those bodies which the eye perceives, is in reality a compound of others still more minute, just as those of larger size are demonstratively compounds of others less than themselves. To prove this in a tubercle of some size is a matter easily accomplished: when it is intersected in different directions, the planes exposed present very generally, even to the unaided eye, thin layers or partitions of a nature different from the larger portion of the body, which traverse the yellow surface of the plane in an irregular manner; sometimes these divisions are very dark, and consequently very distinct, at others their colour is less decided, but they can still, at least very generally, be recognised. There can remain little doubt that this appearance is caused by the intersection of the body of the tubercle by numerous thin layers of the compressed pulmonary texture, the same in fact which at an earlier stage intervened, in a healthy state, between many distinct small tubercles before their approximation, and that consequently, when we talk of a large body of the kind, we in truth allude to one which is constituted by very many less than itself laying in juxta position, but very often in some measure separated from each other by layers of lung, which if they had not approached so closely, would have formed a distinct boundary to each. When we now follow up the examination, by using a glass of moderate power, other partitions which were not before visible are brought into view, and those come out in greater and greater numbers as this power is increased; and though putting forth no pretensions to any great dexterity or experience in the application of the microscope to such inquiries, it still appears to me that we readily acquire from it an acquaintance with the fact, that the plane of an intersected tubercle presents septæ or membranous divisions associated with its proper matter, to an extent infinitely minute, thus leading us to infer, with great probability, that a large body of this kind is *ultimately* composed of innumerable small particles, surrounded by a delicate membrane; and *proximately* of somewhat larger nodules, the result of a union of the former—divided by partitions formed of compressed lung, which are sometimes appreciable by the unaided eye, and sometimes, only come into view when glasses are employed.

The compound nature of tubercle, and the interlacement of delicate membranous filaments with its most intimate corpuscles, is supported by the appearances which these bodies present under the glass in their early stages, not less than it is by examining the internal structure of those of larger size. When a minute tubercle is submitted to the microscope, it is readily perceived not to be the homogeneous nodule which it appears to the naked eye; it presents a round surface, made up of other round particles or molecules which adhere together by a delicate filamentous membrane; occasionally these molecules appear to run in lines forming a chain of soft hair-like threads, with round swellings at certain distances, so interlaced with other lines of the same description as to render their isolation under the eye, very imperfect, but still distinct enough to convey the idea of a body formed of innumerable molecules adhering to each other, not as a consequence of their own cohesion only, but through the intervention of a tissue which forms their seat.

These observations are not put forth with any intention of claiming for them originality, but merely as confirmatory of those before made by Dr. Khun. On submitting tubercles in their first stage to the microscope, he found them to possess a mammillary aspect, to consist of an agglomeration of irregular yellow corpuscles, united to each other by an extremely delicate filamentous membrane, to which he gives the name of "tuberculous tissue:" this membrane is, according to him, frequently spit up, along with detached fragments of the softened tubercle, and may be detected in the expectoration: he describes this tissue as having for its basis thread-like lines of gelatinous appearance, ramifying and uniting with themselves, and contained in a sort of mucous membrane envelope; round these threads and within the mucous envelope are spread a great many albuminous globules, which appear to be productions of the thread, which thus establishes a connexion between the globules.

The idea does not seem to have occurred to this observer, that the globules here described might possibly be extremely minute particles of a white matter, contained within the calibre of small vessels, whose coats formed the delicate line connecting them; and yet if he had endeavoured, from imagination only, to describe the probable appearances of the inner membrane of an air vesicle, whose minute vessels were obstructed by an abnormal deposit, he could not have conveyed a clearer picture. First we have announced the existence of a delicate filamentous line, studded along its course with minute albuminous granules, and these surrounded by a mucous membrane envelope; and finally we find these lines forming constant unions and re-unions with each other after the manner of arteries. What can better describe the probable appearances of a series of transparent vessels, obstructed at certain points by the presence of a white substance, ramifying on a mucous tissue, and forming by the coalition of many molecules a larger body than themselves—in fact, a tubercle of appreciable size?

Considering the many fallacies unavoidably connected with all microscopic inquiries, I do not indeed assert that the correctness of this conclusion is fully established by these observations, but only, that when taken in combination with the arguments before brought forward, they go far to support the probability of the opinion.

I have now, in further prosecution of the subject, to allude to other circumstances tending to the same end, which seem to render its truth, if not certain, at least probable in so high a degree as to demand further investigation, especially when we consider, that it appears to lead to important practical inferences, as I hope yet to show in a subsequent part of this work.

We know with certainty, that in the natural healthy state, the capillary vessels of either circulation bear such a relation to the particles of the blood, that this fluid traverses them with great facility, and that its free transit is essential to a due performance of every function of the animal economy. If substances, such as alum, which act chemically on the blood, so as to induce a partial thickening or coagulation, are blended with it in the living state ; or if a thick fluid, such as oil, which exerting no chemical influence, acts mechanically alone, be injected into the veins, death very speedily ensues ; and there are strong reasons to believe that this event is produced by an obstruction caused in the extreme vessels of the pulmonic circle. The intensity of effect probably depends here on a general deterioration of the whole mass, and a consequent general impediment throughout the pulmonic vessels ; but if the obstructing cause acted only partially on some of these alone, it is not an improbable supposition, that a sufficient number would still remain pervious, to continue, though in an imperfect manner, the purposes of respiration, and as following them, the purposes of life.

I have already said, that the normal facility of transmission through the pulmonic capillaries, which we presume to be interrupted in the above examples, depends on a due mechanical relation between their calibre and the size of the particles of blood. This relation may hence be altered in one of two ways—either by the abnormal magnitude of certain particles contained in the blood, or the abnormal smallness of the vessels through which a healthy blood requires to pass ; and it seems quite evident, that should these two conditions co-exist, the effect will be produced with greater facility and to a greater extent : thus, if the vessels be preternaturally small, the blood remaining healthy, it may lead to the same result as if the blood contained abnormal particles, while the vessels presented a natural size, and this result would undoubtedly be induced with greater certainty, if both the vessels and blood deviated from their healthy state. It is certain, that an *entire* absence of this relation between the blood and its vessels cannot exist in the living animal, otherwise it never could have lived at all; but it by no means follows from this, that there may not be inherent in its organization a certain abnormal conformation of the extreme arteries which confers a tendency to obstructions of the kind named, which may be called into action, or in other words determined, by changes in the blood, a fluid, we are fully certain, liable to constant variations in its chemical constitution as well as mechanical consistence.

No doubt a full elucidation of this question could only be acquired from an extensive series of experiments made on living animals, but independent of the various difficulties by which such an investigation is beset, when even healthy subjects are employed, it becomes still more difficult, if not impracticable, where diseased conditions in all their phases ought to form the objects of inquiry. I conceive therefore that experiments directed to this end, made on the lungs in a dead state, may in some measure at least answer the purpose; and if it can be shown by these that there exists a vast difference in the facility of transit, of the same compound injection, through the capillaries of different lungs—that this facility is least where the evidences of tubercle are most distinct, and much diminished where the evidence of *tendency* to tubercle, as derived from other sources, is considerable—then do I conceive that facts have been adduced corroborative, to a great extent, of the opinion on other grounds maintained, that tubercle is the result of obstruction in the minute vessels of the pulmonary circulation. A desire to examine the condition of the pulmonary vessels when the lungs were beset with tubercles, first led me to attempt their injection in the early as well as advanced stages of phthisis; and I was afterwards induced to extend the examination to lungs in other states of disease, in consequence of certain changes wrought on the injected matter, from which it appeared, that pathological inductions of some importance might be made. All coloured substances employed for the purpose of injection necessarily consist of two parts, a vehicle of some solid matter readily liquified by heat, with which a coloured powder is mechanically blended. Of these two portions, it may be presumed that the particles of either vary in size; those of the colouring matter, when metallic oxides or sulphurets are employed, being larger than those of the vehicle which contains them; and just as the two ingredients of such a compound fluid are susceptible of detachment by a process of mechanical filtration exterior to the body, so are they in some instances separated in a similar manner during their transit through the capillary vessels of the lung.

The injection employed in all the cases yet to be narrated, was composed of well-strained mutton suet rendered less hard by the addition of a fifth part of pure olive oil, to which was added as much finely levigated vermilion as was sufficient to communicate a bright red tint. In each instance, the matter was thrown in by the trunk of the pulmonary artery, the lung having had its temperature previously raised by immersion for some time in warm water. My attempts were first made on lungs in the later stages of consumption, the results of which, will come more properly under notice at a future period, when we treat of the nature and causes of that reaction of the lung towards the system, which is most marked in an advanced stage of the disease: here, I have to refer to those phenomena resulting from the employment of injections in its early stages, which more immediately bear on questions connected with the predisposition of the lung to form tubercle, the nature of those bodies, and the immediate mode of their location.

When a composition similar to that which I have already described is thrown with some force into the pulmonary artery, the entire vessels of the organ which belong to the smaller circulation are readily filled by the injection, which passes with such facility that it is often returned to the left auricle of the heart, having traversed the trunk and branches of the artery, the final extremities into which these divide, the incipient roots of the pulmonic veins, and finally the veins themselves.

In some cases no change is wrought, during this transit, on the matter employed; what fills the left auricle being, in colour and consistence, similar to that which is contained in the trunk of the artery where it first entered: in others, the ingredients of the injection sustain a separation, and the larger branches of the pulmonary veins—or, if the process is successfully executed, the left auricle itself—contain a concrete tallow, more or less deprived of its colouring particles, being either altogether paler than the original injection, or composed of white and red portions, of different shades, mechanically intermixed.

On cutting into lungs which have been so treated, it at times happens that the vessels intersected, whether they be arteries or veins, are filled with injection altogether similar in colour; but in many, while some of the appreciable vessels contain red matter, others are filled by a tallow which is more or less pale. The former are the branches of the pulmonary artery, the latter those of their corresponding veins. In such cases it is obvious that a separation has taken place between the vermilion and basis of injection during its transit from the artery to the vein; nor can we account for it on any other principle than this—that the capillary extremities of the one series of vessels, or the incipient radicles of the other, had acted mechanically on the compound injection, permitting the particles of liquid tallow to pass, but intercepting the molecules of colouring matter with which these were before associated. The physical cause of this filtration seems to depend on the relatively larger size of the colouring particles, when compared with those of their fluid vehicle, in consequence of which the one set pass through canals which the others are unable to traverse. I have already stated that in some instances the injected matter passes back by the veins unchanged; while the veins which convey the blood from others are filled with whitish, or white tallow alone. On making a section of the lobe transversely, the cut surface, when viewed by the unaided eye, or still better through a glass of moderate power, presents a plane of various shades of redness; studded with round specks, of different sizes and colour, from deep red to pure white—the cut ends of pulmonary arteries and veins intersected transversely to the axis of their cylinders; those whose colour similates the shade of the original injection will be found to be arteries; those the colour of whose contents differs from this—veins.

These appearances indicate, without any doubt, a retention in some instances of the colouring matter by the capillary vessels; but they do more than this, and seem to show, that in some examples, *particular sets* only of those vessels effect the separation, while others in the same subject permit the entire injection to pass unchanged; or in other words, they would appear to demonstrate, that not only does there exist a marked distinction in the resulting phenomena from injections made on the pulmonic blood vessels of different persons; but also that distinct portions of the lungs of the same individual, when so treated, produce on the material employed very different effects.

These facts seem established by the preceding observations; but it still remains a curious question to investigate, whether the relative facility with which the constituents of an injection are separated in different cases, is susceptible of being connected with appreciable peculiarities of constitution, in the individuals whose lungs form the subjects of examination.

I am aware that before this point could be with any certainty established, it would require that the investigation be pursued through a large number of cases, and that consequently what I have to say on the subject is more to be considered as proposing a question eminently entitled to further consideration, than as one by any means fully determined, by the comparatively limited inquiries which time and opportunity have enabled myself to make. It has however been in my power to subject the lungs of 38 persons to the process named; a few of these were healthy—the larger portion diseased in different ways, and to a greater or less extent. Of the apparently healthy, some were obtained from subjects who did not exhibit any of the signs by which a strumous constitution is generally recognised, and some from those in which the scrofulous tendency fully appeared, although the lungs themselves were as yet sound. Of those which were obviously diseased, the large majority were occupied by decided tubercles in different stages; and four were cases, in which the organ was simply consolidated by previous inflammation.

In examples where no perceptible disease of the lung existed, and where the individual showed no signs of a strumous constitution, the red injection entering by the pulmonary artery, returned through the corresponding veins, of an unaltered appearance, having minutely filled the extreme vessels.

On making sections of such lungs in various directions, the tallow contained in the branches of the vein, as well as artery, was altogether similar in appearance, the minute vessels simply retain what is sufficient to fill them, but without producing any separation between the white vehicle and coloured particles.

A preparation of this kind is exhibited in Plate I. Fig. 1. It presents one uniform red surface, varied only in intensity of shade by the sections of the injection contained within the calibre of the larger vessels, being of a bright hue as compared with the duller red of their final branches,* and appears conclusively to show that, in this instance, the white basis of the injection and the colouring matter associated with it had not been detached from each other during its interchange from artery to vein.

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* The cause of this difference of shade is sufficiently apparent. When the section of a large vessel is made, we bring into view the surface of the pure injection contained within its cylinder.

A brief notice of the age and other circumstances of the patient, from whom the lung in this example was taken, will be found in the explanations accompanying the plates ; I here only remark, that it affords a fair illustration of the phenomena observed, generally, if not always, in lungs which being healthy as organs, were also derived from persons who showed no signs of a strumous constitution.

To contrast the appearances here shown with those presented by lungs similarly treated which were derived from strumous subjects, but in themselves free from tubercle, is in practice not very easily accomplished, inasmuch as tubercular deposition very seldom occurs in other organs while the lungs are free from it.

Fig. 2 of the first plate, however, exhibits the section of a lung taken from a person who presented this somewhat unusual combination. The immediate cause of death was strumous softening of the brain, which succeeded an attack of measles : the glands of the neck and abdomen were both en-

When the knife intersects only the capillary tissue of the organ, the coats of its minute vessels intervene between almost each particle of the injection, and the animal matter thus interwoven, in some measure, dulls the brightness of its tint.

larged—but the lungs, so far as could be traced, did not contain any tubercular deposition—were free from pleural adhesions or other signs of inflammatory action, and crepitous throughout: the injection pervaded the entire organ and filled its capillary circulation with much minuteness, passing readily from the artery to the vein. On examining the substance of the organ, afterwards, the majority of pulmonary veins were filled with red injected matter, altogether unaltered; while in some instances they contained the white portion only, the vermilion having been deposited in the extreme vessels of the arterial tree. Of some of the veins so circumstanced, the contents were entirely white—in others again, faintly tinged with red—and in others still, white and coloured portions, distinct from each other, occupied the canal, as if its contents had been conveyed to it by smaller tubes, some of which carried one description of injection, and some, another.

The figure represents a fair average section of the lung now under consideration, made in a direction parallel with the axis of the lobe: the white veins are few, but sufficiently numerous to demonstrate the decolourising process which had taken place at some points within its substance, and show the difference in this respect between it and the one shown in Fig. 1.

Fig. 3 of the same plate represents the section of injected lung taken from a child five years of age, the member of a highly strumous family, and itself presenting all the characters of this diathesis. The immediate cause of its death was marasmus, connected with mysenteric disease, and the glands of the neck were considerably enlarged; the lungs contained in their upper portions on both sides a good many scattered tubercles, none of which had as yet passed into their softened stage. Similar results, only in a much greater degree, are here obtained from the injection, as in the preceding instance. The section shows numerous considerable branches of the pulmonary artery cut through in a transverse direction, which contain the tallow highly coloured, and many pulmonary veins, whose cylinders are occupied by the same matter, entirely white; the vermilion being retained in the bright red tissue of the lung, which forms the principle portion of the plane which the engraving exhibits.

Fig. 4 of the first plate affords a correct representation of a similar section, made from the lung of a person aged five years, in which tubercles in different stages existed to a great extent, and whose possessor during life was characterized by all the marks of aggravated scrofula.

In this case, the child could not be strictly said to have died of phthisis, tubercular softening of the brain being the *immediate* cause of its dissolution; for though the lungs were loaded with tubercles, and the germs of a disease thus existed which must probably in the end have destroyed life, few marked symptoms referrible to the chest existed. The subject being thus one, in whom all the characters of struma were combined with decided and extensive deposition in the lung, offers a fair contrast in the ascending scale between the two extremes of healthy and diseased conditions, in their relative decolourising powers, which at present forms the object of inquiry; and it will be found, that the quantity of injection deprived of its colouring material, and the number of veins containing the white portion only, are considerably larger than in those specimens from which the other figures of the engraving were derived.

Fig. 5 of the same plate represents the injected matter used in all the cases, as derived from the trunk of the pulmonary artery after the removal of the pipe employed, and shows the colour of the tallow as prepared for use.

The sixth and final figure of this plate affords a specimen of the injection taken from the left auricle of the same subject, after it had traversed the pulmonic circle of vessels.

The case from which these were derived, is that which furnished the section delineated in Fig. 3; they are merely used in confirmation of a fact already sufficiently insisted on, and appear conclusively to show, not only that the pulmonic capillaries in some instances separate the ingredients of a compound injection, but that distinct vessels of the same lung affect this in a different degree.* On the whole, my conviction is strong, that by these trials the physical fact is in a great degree established, that between the lungs of a healthy person and those of an individual actually labouring under consumption, a long gradation exists in the facility with which particles of a certain size traverse their capillary vessels; and that this affords data from which valuable pathological conclusions may be deduced.

It yet however remains to be seen how this applies to the formation of tubercles, or throws any light on the nature of that condition which has been conceived to establish a *tendency* or *predisposition* to their evolution.

An explanation of the phenomena before described must necessarily be sought for in two circumstances, which mechanically modify the appearances resulting from injections in the dead subject. The first of these is, that the particles of vermilion being positively larger than those of the liquid fat with which they are associated, and under certain circumstances relatively larger than the terminal vessels towards which they are projected, become retained within their cylinders, while the white tallow freely passes. And the second is, that all the capillaries of the same lung are not so constituted as to effect this separation, some sets retaining the coloured matter, while others permit it to pass freely into their corresponding veins. What here happens after death, when extraneous compound injections are employed, may, it is conceivable, occur during the transit of blood through the same vessels while life remains—provided, from any cause, particles preternaturally large be contained in this fluid; under which circumstances tubercle of the lung would owe its origin to the retention within the cylinders of the pulmonic capillaries, of a matter previously existing in the blood, and thus result from a mechanical process analogous to that by which the particles of vermilion are retained in the examples given; the difference consisting not in the nature of the action, but in the quality of the obstructing cause; this being in the one case, a particle of vermilion; and in the other, a particle foreign to healthy blood. On such a supposition, the formation of pulmonary tubercle depends on a reversal of that due relation which the particles of blood bear to the calibre of the capillaries; a change which, as in the case of injections, may depend on two circumstances—either an increase in the magnitude of the particle, or an unwonted smallness of the vessel; the one we must consider to be in a great degree inherent and unalterable, for it is not easy to suppose that the vessels of the living body change at different times: the other, we may with equal certainty consider as liable to constant variations, because we know as a matter of fact, that the blood changes in composition from the action of many causes. If then we are prepared to admit the occasional existence in this fluid of globules or particles foreign to its healthy nature, it is obvious that these may or may not be retained in transit through the minute vessels, just as they present a normal or abnormal conformation; and hence it follows that, in some cases, a blood highly charged with the matter of tubercle, may yet fail to produce tubercles themselves—using this term in its common acceptation; while in others, a blood much less highly charged may fully determine the disease which depends on their presence. Here, I conceive, we arrive at a point which throws some light on the causes of what we name a *tendency* to tubercular disease of the lung—a condition not necessarily followed by its development; as also on that which immediately calls this into action, and fully establishes the local malady.

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* Several reasons will readily suggest themselves for an author avoiding the multiplication of plates, unless this is imperatively demanded for the illustration of his subject. I have consequently selected only such examples as by affording a tolerably marked contrast, seem best calculated to effect this purpose. I may however add, that the figures, though few in number, are faithfully selected from a larger amount of cases in which the experiment has been made: that throughout the series a marked difference has appeared in the extent of separative influence exerted by the pulmonary vessels of different subjects; and that this has borne a strict relation either to the existing tubercular disease of the organ, or to the constitutional tendency to phthisis deducible from evidence less conclusive.

The whole tenor of the preceding remarks then tend to the conclusion, that this predisposition is traceable to an unusually small formation of the minute arteries of the pulmonic circle, which doubtless varies in degree as well as in the number of vessels which present it in the same lung; while the determining cause of the disease is to be sought in that condition of the blood which, being produced by causes fluctuating in their nature—but acting through it on an organization inherently imperfect, leads finally to those results by which the material cause of tubercular phthisis is established. To such solution of the question there are two specious objections, which at once present themselves to our notice. The first is, that our theory leaves it unexplained how such an abnormal condition of the pulmonic capillaries should occur at all; and still less, how it should only be found in some portions of those which pervade the same organ. The second is, that the very existence of the globules which we have supposed to be the determining cause of the disease, is so far as we have yet gone, assumed, but not proved.

To the first of these objections I can only reply by resting on the fact, that analogous and well marked peculiarities of organization are constantly manifested by individuals, in many parts of the body, and further that these are without doubt transmitted downwards from one generation to another, precisely in the same manner as a tendency to strumous or consumptive diseases is transmitted. By the colour of the eyes—the form of the head and features—the strength, or weakness, of the muscular system—and numerous other marks, the child is frequently identified in structure with its parents; and if it be said that such occurrences, though common, are by no means universal, I answer it is just as usual to observe them as to find the child of a consumptive family become itself consumptive.

The analogies between parent and offspring, which I have alluded to in illustration of the subject, are so clearly marked as to be at once recognised and at once admitted; but if we reflect on the matter, they must be considered to result from certain unseen peculiarities of internal structure, by which the external similarity is produced, constituting as it were a minute special anatomy belonging to different races and families of mankind; and why therefore should we hesitate to believe that this may influence the minute organization of the extreme vessels?—there in truth seems no more reason to question the possibility that a parent, possessing pulmonic capillaries unusually small, should transmit this conformation to his descendant, than that such descendant should be fashioned in the same mould as the beings to whom he owes his existence, in such points of structure as more readily come under our cognizance. If it again be objected, that in the case before us the mal-formation of the vessels is limited to part only in the same subject, I answer by simply appealing to the facts already noticed, without any attempt to assign a reason for the difference: in the present state of knowledge, I fear we are continually destined to encounter difficulties of this nature, and it is not less beyond us to explain why, in certain individuals, some of the capillaries of different external parts should fail to admit the red globules of the blood at all, or in others to receive them in unusually large numbers, than it is to show how special portions only of the lung should be pervaded by pulmonic vessels, in whose structure and action analogous differences exist.

The induction which appears warranted by the preceding remarks is this ;—that the pre-disposition to form tubercles in the lung depends on organization—that the process by which these bodies are immediately evolved is of a mechanical character—and that this is finally determined, or called into action, by an unhealthy condition of the blood—produced by causes still obscure, but which act by charging it with unassimilated particles or globules of a kind foreign to the natural constitution of that fluid. We have yet, however, to see how far this is borne out by our existing knowledge on the subject, and such an inquiry evidently involves two distinct points. First, whether the presumed matter of tubercle is demonstrable in the blood under any circumstances? and if so, whether, in the second place, we can in any way account for the primary errors of action which lead to its presence? The last question, according to the hypothesis supported, clearly involves a consideration of such phenomena of the tubercular diothesis as are not referrible to abnormal conditions of capillary organization.

The older physicians who have recorded their opinions on consumption, especially during the ascendency of a humoral pathology, seldom entertained a doubt that this disease, in common with many others, owed its origin to certain humours present in the blood; their ideas on the subject were however vague and hypothetical; they seldom thought of inquiring how "the humour" came there, and thus restricting their views to causes of a secondary kind, altogether lost sight of that reciprocal action and re-action between the solid and fluid constituents of the body, on a due reference to which all just and enlarged views in physiology, and consequently in pathology, must necessarily depend. The wild assumptions consequent on this fundamental error led to a general re-action in the whole tone of medical thinking, which it may be perhaps admissible to say has been hostile to its rational advance.

Be this however as it may, one thing at least is certain, that whatever be the result, men now tend to consider a humoral pathology, under certain restrictions, not quite so absurd as they at one period conceived, and consequently a strong impression prevails that the matter of tubercle exists in different organs in a liquid form. Andral "thinks it probable that tubercle is in the fluid state at the moment of its deposition;" founding this cautious opinion on having at times observed, in lungs filled with tubercles, white points composed of a fluid substance, like a drop of pus. His countryman, Crouveilhier, advocates the same view in a more decided manner, and among ourselves, Dr. Carswell recognises the blood as one of the localities in which tubercular matter is occasionally found.

The strongest facts however, in confirmation of this belief, are undoubtedly those furnished by the microscope ; the concurrent testimony of numerous inquirers no longer leaves it in doubt, that under some conditions of disease actually formed, or in some states approaching to disease, there exist in the blood certain particles which are altogether different from those which belong to it in health ; the name by which we may designate these particles is but of trivial moment—they have usually been termed "pus globules ;" but as we yet know little of them beyond the fact of their existence, this term is perhaps objectionable, as it identifies with a definable fluid, a matter, whose nature is not as yet by any means determined. For our present purpose it is more important to know that they have hitherto been found chiefly, if not solely, in the blood of persons who labour under some of the various forms of cahexia, and hence, frequently in cases where no decided symptoms of phthisis have been present ; but on the other hand, where the tendency to this disease is strong, or its presence unequivocally announced by decided signs, they will, I am disposed to think, be seldom absent ; and they thus appear to constitute one of the elements demanded for its full and perfect formation.

On such a question it behoves us to speak at present with a due degree of caution, but still I cannot avoid expressing a belief, that the demonstrable existence of these particles, or "pus globules," in the blood, and their very general, if not universal presence in that of persons labouring under phthisis, goes some length in support of the hypothesis which it has been the leading object of this chapter to advocate; they form as it were the intermediate link between that primary aberration from healthy action by which they are themselves produced, and the local affection of the lung which they partly originate; they first appear as the effect of an antecedent cause which is extremely obscure; they then change their character and become the immediate cause of the local lesion, where that predisposition exists which we presume to depend on an erroneous formation of a portion of the pulmonic capillaries.

But the circumstances under which tubercles of the lung arise are by no means fully determined, even supposing their matter to exist in the blood, and our theory of its location to be correct; for it is evident that its appearance there necessarily involves the operation of some anterior morbid change, of which this matter is the result.

Many circumstances encourage the conviction that this error, which constitutes so important a part in the phthisical constitution, is connected with some one or more of the various processes by which aliment is finally converted into blood, and this conducts to the inference, that the primary cause of tubercular consumption has its seat in the digestive organs.

In employing this term, however, I could wish it to be taken in a wider sense than that in which it is generally used; between the first reception of food into the stomach and its final identification with blood by the respiratory function, many and elaborate processes occur; if any one of these fail, the healthy result is not perfectly attained, and hence disease may not only depend on the undue execution of any one, but diseases of a different nature may arise according to the particular part of the process which is deranged.

It would appear probable that the primary error of digestion which conduces to the establishment of phthisis, lies at a point in its series of actions anterior to that of final sanguification; for as the blood contains abnormal constituents, these are most probably thrown into it before reaching the lungs; but at what point they originate, or by what immediate change they are produced, are questions which it seems to me we are as yet unable to answer. But there is obviously a wider view of this matter to be taken, founded on a morbid action and re-action between the digestive organs and the lungs; if the former prepare the chyle, so do the latter sanguify it, and hence, while a lung already impeded may be assumed incapable of duly elaborating a chyle properly constituted; so on the other hand, it is probable that a similar result may follow from the presentation of a chyle improperly formed, to lungs in themselves perfect; it is this reciprocal dependance of organ on organ, and function on function, throughout the whole animal economy, which renders it so difficult in all examples to determine the primary error of action from which diseases spring; and in no case is this attended with greater difficulty than in phthisis; if for example we say that the digestive organs are its original seat, the question still remains how this occurs, and even admitting its occurrence, how this should produce consumption, certainly not always the result of indigestion; if again we say that the nutritive powers of life are interfered with, in consequence of an impeded condition of the lung influencing the early organs of digestion either in their structure or functions, the conclusion is at least a rational one, but it unfortunately throws no light on the question of how this impediment was produced—the very point which it would seem desirable to solve. But though for such reasons it is difficult, nay impossible, to do more than conjecture what may be the first error which conducts to phthisis, and though I can offer no better reasons than those already given for the opinion that this occurs in the earlier organs of digestion, we can, I think, with more certainty and with some advantage trace how, an impulse once given, the disease continues to advance.

Its progressive character forms one of the most marked phenomena of consumption; very seldom, if ever, do we find the lungs suddenly loaded with a large or fatal evolution of tubercles; on the contrary, the complaint begins in an obscure, insidious manner, a circumstance rendering its early diagnosis so extremely difficult; in very many cases, even where the general aspect of the patient, his hereditary tendencies, and other similar indications have engendered a strong suspicion, the physical signs and general symptoms are so equivocal and obscure that little positive certainty can be obtained, and it is hence probable that the deposition of tubercles commences, and that these exert a certain amount of influence over the organs and actions of the body before their presence is announced by decided evidence.

We talk of a "phthisical person" every day, and yet would be guarded in asserting that he actually laboured under consumption; he exhibits perhaps all the characters of the diathesis, but the symptoms are still insufficient to justify more than a strong suspicion, and the physical signs are still less decided. In such examples—and they are very common—it is a curious and interesting question to solve, whether the slight symptoms present are to be attributed to the influence of a few tubercles actually deposited, or to the operation of the anterior functional disturbance to which they owe their production.

In the great majority of such cases it appears probable that both causes operate in producing the condition which attracts our notice, though of this no certainty can be attained; but however this may be, one fact is certain, that the disease is progressive in its nature and gathers strength as it proceeds, augmenting with a gradually accelerated force till the final acme is attained; and thus the truth of Laenec's observation is borne out by constant experience, that the softening of one set of tubercles becomes the apparent cause of the speedier deposition of another—the disease thus appearing to feed as it were on itself. For this there must exist a reason, and no explanation seems more probable than that which refers it to an action and re-action between the lungs and abdominal viscera, organs which constitute the extreme ends of the nutritive chain. Supposing a few tubercles deposited in a lung before healthy, they will undoubtedly in correspondence with their number lessen its sanguifying powers, its capability of identifying chyle with blood will be diminished, or its function, as remotely ministering to nutrition, impaired, while coincident with this there must occur also, a certain decrease in its capacity to arterialize the blood, on the proper execution of which act, the earlier nutritive organs are themselves *immediately* dependant. If we follow this chain of causation a few steps further back, to what does it conduct us? The early error in digestion, which we have assumed as the original one, induces an unhealthy condition of the blood, and this, under certain circumstances, induces an obstructed condition of the lung; this organ then assumes its turn of re-action, and transmits to the digestive apparatus a blood imperfectly prepared; these again feel the shock of this re-action, and become still less able to execute their healthy duties, in consequence of an organic change in remote organs, originally excited by themselves. What happens when a few tubercles occupy the lung, must occur in a greater ratio when many are present, inasmuch as they exert an influence great in proportion to their number; but the probable explanation of how the pulmonary affection once established, comes through the medium of the blood to engender the elements of its own increase, and of general constitutional evils, is more properly treated of in the succeeding chapter, in which the phenomena of morbid pulmonic re-action on the system is discussed.

It has been thus my endeavour in this chapter, to connect as far as may be, the location of tubercle in the lung with its primary, though remote cause.

The immediate and intermediate causes of the affection appear in some measure elucidated by the previous remarks, but I am free to confess that the original error is to a large extent conjectural; it is however according to my experience true, that in all cases of consumption—or, taking a wider range, in all examples of the strumous constitution, there exists an early stage, characterized by symptoms which indicate a depraved condition of the digestive organs. The form which this usually assumes is that of irritation of the canal, which begins with the mouth and ends with the anus; indicated by the diagnostic signs of red prominent papillæ of the tongue, protruded through a white coating, unhealthy abdominal secretions, and in children especially, a tumid abdomen; but we should ever bear in remembrance, that though the condition on which such symptoms depend, constitutes in a great degree the early element in tubercular disease of the lung, as of tubercular disease in other organs, it is not necessarily followed by strumous deposition in any situation, and that hence our ideas are but vague and inconclusive, when we refer the production of local struma to this cause, and to this alone.

My present object being to treat of phthisis only, it does not fall within the scope of this book to enter on the question of struma generally. I conceive, however, that the affections are strictly analogous—excited by similar extraneous causes—dependant on the same errors of action—and locally determined by similar inherent vices of organization. Why then do not all persons who possess phthisical lungs also present the signs of general struma? or conversely, all persons with swelled glands, and the like, become phthisical? I answer that the two affections frequently, nay generally, co-exist—that when tubercles are formed in the lung, they usually exist also in the system, more especially in the glands—those very organs in which the convolutions of the aortic capillaries are greatest, and where as a consequence obstructions are most likely to occur. Why pulmonic and systemic struma do not *always* co-exist, is a question not at present to be answered, except hypothetically.

May it not be possible that, in the structure of some individuals, a blood passes freely through the vessels of the lung, which is retarded in those of the system ? If so, we may conceive that a blood deteriorated by the action of similar causes, may in one case produce general struma, and in another phthisis—or in a third still, that the two affections may result. On such a supposition we should expect the location of tubercle in the lung to be the more common result, because the pulmonic circle of vessels is first traversed by the new constituents of the blood, and this seems to be the truth as attested by general observation.

CHAPTER III.

THE EVILS WHICH RESULT FROM MORBID CHANGES IN AN ORGAN REGULATED BY ITS PHYSIOLOGICAL IMPORTANCE—APPLICATION OF THIS PRINCIPLE TO THE LUNGS—CHANGES IN THEM INFLUENCE THE SYSTEM, IN A DEGREE CORRESPONDENT NOT ONLY WITH THEIR EXTENT, BUT NATURE—OF ALL, TUBERCLES PRODUCE THE GREATEST EFFECT—PROOFS OF SUCH OPINION—REASONS FOR THE DIFFERENCE—PHYSICAL CONDITIONS REQUISITE FOR HEALTHY RESPIRATION—FREQUENT ABSENCE OF ONE OF THESE IN CERTAIN PORTIONS OF A CONSUMPTIVE LUNG—DIFFERENCE IN THE CONDITION OF VESSELS IN LUNG SOLIDIFIED BY TUBERCLE ON THE ONE HAND, AND INFLAMMATION ON THE OTHER—VARIETIES EXPLAINED—PROBABLE CAUSES OF DIFFERENCE SUGGESTED—INFERENCES AS TO THE PHYSIOLOGICAL EFFECT OF SUCH PHYSICAL CONDITIONS—THOSE PRIMARILY ACT ON THE ARTERIAL BLOOD—SECONDARILY, THROUGH IT ON THE ORGANS—REASONS WHY THE SAME CHANGE IN THIS FLUID DOES NOT RESULT FROM OTHER DISEASES OF THE LUNG—REASONS WHY TUBERCLES OF THE LUNG DO NOT ALWAYS PRODUCE IT AS IN EXAMPLES OF "LATENT PHTHISIS"—SOFTENING OF TUBERCLES A SANATORY EFFORT INDUCED THROUGH AN INFLAMMATORY ACTION OF THE NEIGHBOURING TISSUES—WHY NOT ALWAYS SUCCESSFUL—WHEN SO THIS DEPENDS (AT LEAST IN SOME MEASURE) ON THE CLOSURE OF PULMONIC VESSELS PREVIOUSLY OPEN—LIMITED POWER OF ART IN PRODUCING SUCH RESULT—IMPORTANT PRACTICAL DEDUCTION—CAUTIONS AGAINST ACTIVE DEPLETION IN PHTHISIS—SHORT RETROSPECTIVE SUMMARY OF THE OPINIONS HITHERTO ADVANCED.

Having, in the former chapter, endeavoured to connect the matter of tubercle with the primary functional disorder which produces it, and attempted to show how this matter, through the medium of the blood, comes to be deposited in the minute arteries of the lung, it yet remains to examine into the causes and character of the morbid re-action, exerted by lungs so occupied, on the organs and functions generally.

It is impossible perhaps, as I before remarked, to separate entirely the two phases of the complaint, or refer with precision a certain number of its phenomena, to the operation of those causes by which tubercle is produced, and another set, to the influence exerted by the impaired condition of the lung, produced by its presence; this difficulty is greatest in the early periods of the disease, in consequence of our still imperfect means of recognising the existence of deposit in its very first stages; at a later period this difficulty is much diminished, for the morbid re-action of the lung then becomes strongly marked; but in both, it appears to me, the distinction can to a certain extent be made, and considering the question in a practical point of view, I feel well convinced, is pregnant with no inconsiderable advantages. It is a law in physiology subject to no exceptions, that the evils which result towards the whole body from the derangement of any particular organ, are proportionate to the influence which it exerts in the general economy, and as the function performed by the lungs does not yield in importance to any other, not even excepting those of the heart or brain,—so it is easy to assign a reason, in general terms, for the morbid re-action propagated to other parts as a consequence of its derangements; though it is not quite so apparent why the intensity of this should be fully as much determined by the nature or character of the local affection, as by the extent of lung which it involves.

While nature, in sustaining the perfect unity of the body, has assigned to each organ a relative degree of importance as regards the whole, she has also provided, in an admirable manner, for obviating the evils which arise from impediments to the due execution of particular functions; and this provision seems strongly marked, in proportion as the function is necessary to life. To ensure this end, each important organ possesses a capacity of functional activity greater than the usual wants of the body require; a constant reservation of power is thus always at hand, to meet extraordinary emergencies, which can readily and speedily be called into exercise—the body thus, as it were, living faster for a time, under the operation of many causes, and resuming its natural equilibrium unhurt when these have ceased to operate. As this happens during the healthy state of organs, so does it also happen in their diseased conditions; and hence we often find that the function of a part continues to be exercised, and this even in tolerable perfection, when considerable portions of it have, as a consequence of structural change, been obviously abstracted from an execution of the duty which in the healthy state it fulfils.

To no organs do these remarks apply with greater strictness than to the lungs. Destined to antagonize the systemic capillaries, by producing a quantity of arterial blood equivalent to that which these consume, the lungs possess a capacity of executing this important act, to an extent infinitely greater than they are usually called on to apply; and it is a matter of constant occurrence to find them respond, with great readiness and entire convenience, to demands of other organs, greatly beyond those which are made during the quiescent states of a healthy body.

The same principle which thus applies to them in health, applies to them likewise in disease; and it is no exaggeration to say, that cases present themselves in which very considerable portions of these organs are obviously incapable of executing the purposes of respiration, while arterial blood, the executor of all functions, is produced and supplied with due regularity. I am far from maintaining that no inconvenience results from the abstraction of portions of the lung from the performance of its duty; on the contrary, we find that in every such example, increments of general action, which in a healthy state produce no appreciable change either in the pulse or respiration, will be marked in these by very decided acceleration of each; neither am I ignorant of the fact, that the support of any very high degree of increase in the systemic functions, is often rendered absolutely impossible, in consequence of organic evils of the lung. Nor do I still further hold that even in states of quiescence, when no unusual causes of excitement act, the frequency of the pulse and respirations are not, in relation to the individual, abnormally great ; but I do conceive that this is often so slight as to attract little notice, and be attended with little inconvenience, in persons where there exists unequivocal evidence of obstruction in certain portions of the organ.

The functions whose execution is attended with the greatest inconvenience under the circumstances I have named, are those of animal life, while those of organic life often proceed in every respect with proper regularity.

An explanation of this may possibly be found, in the relative speed with which an augmented quantity of red blood requires to be supplied for the sustenance of acts included under the two order of functions ; the actions of animal life are executed with electric velocity, and their sustaining fluid demanded with corresponding speed ; while the actions of organic life, being independent of the will, and not so immediately under the control of external excitants, make their calls upon the lung in a more equal and steady manner ; the difference, on this supposition, depends on the time within which a specified amount of arterialization requires to be performed ; in one case the demand being sudden, an imperfect lung either fails to execute it, or does so with marked inconvenience; in the other the demand being slowly made, it accomplishes this in a manner comparatively easy—what is wanted in extent of reddening surface being, in some measure, compensated by the time allowed for the transmission of blood over surfaces which are still healthy. Whether such explanation may be considered admissible or not, I am not prepared to say; but in support of the fact towards which it is directed, I would appeal to cases of constant occurrence.

We every day meet with persons, generally elderly persons, who present most distinct evidence of obstructions* in the lungs. In such individuals the functions proceed with great regularity so long as quiescence of the body is maintained, the act of nutrition is performed in a proper manner, and the secretions duly effectuated; but subject such persons to any unusual causes of excitation—let them be called on suddenly to exert their muscular system strongly, or apply a cause which induces unusual mental action—in short accelerate any one of the functions which are peculiar to the animal, and the existing impediment of the lung is at once announced by severe, and at times even dangerous, symptoms ; the breathing becomes hurried and laborious—the pulse inordinately quick—and speedy exhaustion follows. These symptoms, superficially explained by reference to the very vague term "debility," really depend on an existing incapacity of the lung to produce within a stated time a sufficient quantity of red blood to meet the exigencies of accelerated action, and are hence dependant on the same cause as that want of power, or "debility," which attends on partial asphyxia in a healthy subject.

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* By the term "obstruction," I mean to indicate, any cause which interferes with a proper execution of the respiratory function. Whether portions of the lung be so consolidated that air cannot enter at all, as occurs in hepatization, or whether, having entered, it becomes extravasated in its tissues, and fails to undergo those alternations of ingress and egress which healthy respiration demands—as happens in emphysema; or whether finally, a morbid state of the lining membrane of the bronchiæ, and air cells, prevents a due action between blood and air; they all lead to one result, an impaired execution of the changes which the former fluid in a healthy state sustains.

It would appear from these remarks, which refer simply to facts every day presented to our notice, that morbid conditions of the lung commonly exist, which do not exert an influence towards the system at all commensurate to the amount of physical change in organs so important to the whole ; nor is the truth of this at all less, whether we do or do not admit the correctness of the explanation which it has been my endeavour to afford.

While as before remarked comparatively slight evils arise, more especially in the function of nutrition, from various causes which impair the respiratory process, symptoms of a much more decided character constantly supervene, on even a very moderate amount of consolidation, when this depends on tubercular deposit.

The very early signs by which phthisis is suspected, are frequently such as are connected with an impaired execution of the various functions, and this, even before the local signs are in any way distinctly marked. The patient becomes languid and unfitted for his usual muscular exertion, or when he does attempt it even moderately, the respirations are hurried in a manner before unusual, and the pulsations of the heart proceed with unwonted speed;* if he applies his mind intently, and more especially if it be suddenly excited, the actions of his heart and lungs augment in a ratio not naturally correspondent to the amount of action performed; and these symptoms increase in a proportion regulated by the progress of the local disease; so that at length the respirations and pulse are at all times inordinately rapid, even during that perfect quiescence of the body which occurs in sleep. Nor is the early influence of tuberculated lungs less distinctly marked in the organic functions, especially the nutritive. The same cause which impairs the contractile energy of the muscles, impairs also their magnitude and form; they feel flabby in texture, and are diminished in size, and though the general contour of the limbs be perhaps well preserved at this period, this depends on the presence of a serous unhealthy fat, rather than on the bulk or firmness of the muscles which lie beneath.

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* Among the many means which the physician employs in solving the difficult question of the existence of tubercle of the lung in its earliest stage, one of no mean value consists in testing the patient by a stated amount of muscular exertion, and watching the effect which this produces on the actions of the heart and lungs; when we find them inordinately augmented in proportion to the exertion made, it constitutes in my experience a valuable accessory sign, which, coupled with those derived from other sources, may conduct to a correct conclusion.

As the malady creeps on, so do these appearances augment, and it is no unusual event to find considerable emaciation and great muscular debility present, when the tubercles are so few and scattered, that their very existence cannot be with perfect certainty announced.

That these phenomena result, in a great degree at least, from a re-action propagated from the lung towards the system—and are not dependant on an influence exerted by the original derangement of which tubercles themselves are consequences—seems clear, by their not existing to the same extent when organs different from the lungs are occupied by a matter identical with the pulmonary deposit. How frequently do we observe this largely developed in the glands of the lymphatic system, while the muscular and nutritive functions are but slightly, if at all, affected.

But if the conclusion thus appears unavoidable, that the influence resulting from pulmonary imperfection when produced by tubercle, is greater than when it depends on any other cause, the same fact seems also confirmed by morbid anatomy. If we appeal to the results of dissection, even in cases where life has been spun out to the most attenuated thread, and in which, when we judge from symptoms only, the wonder has been how the person could have supported a feeble existence so long, and then refer to the *quantity* of lung which has actually been destroyed, we shall, I apprehend, often find it difficult to connect the intensity of symptomatic effect with the amount of physical disorganization. It is true indeed, on viewing the large excavations which frequently exist—the portions of the organ occupied by tubercles in the early stage of softening—and the still larger portions consolidated either by isolated tubercles in their crude state, or by what is named tubercular infiltration, that the cause of death, and of all previous symptoms, appears at first sight to be sufficiently apparent; but when we come to examine into the actual amount of this lung which is rendered functionally unserviceable, we are led I think to perceive, that this is in many instances by no means so great, as in cases where obstructions arising from other causes have been attended by symptoms greatly less decided or severe; which consequently leads us to conclude, that on the *nature,* not the *extent,* of organic change, depends in a large degree the resulting evils of tubercular phthisis.

Assuming these observations to be correct, it is evident there must exist some reason why the same amount of obstruction in the lung dependant on different organic changes, is followed by symptoms altogether dissimilar in their intensity; and one of the many questions which pathology has yet to solve, in reference to consumption, appears to be connected with this circumstance.

The same method of investigation which I have already considered to throw some light on the *formation* of pulmonary tubercles, seems also capable of elucidating the causes of that intense functional re-action which follows their *location*. This method, it need scarcely be added, consists in injections practised through the pulmonary artery, which by conveying an acquaintance with the physical condition of its important circle of vessels in different states and stages of disease, affords us data of great value from which certain conclusions may be deduced, in regard to the vital evils which such structural conditions engender.

I shall first state the physical phenomena resulting from such injections in the advanced stages of phthisis, and other diseases of the lung; and secondly, the inferences which appear to be warranted by them, as bearing on, what may be termed, the vito-pathology of the disease more immediately under consideration.

I have already shown that the colouring matter of an injection is separated from its other ingredients by the capillaries of the lung, in a degree which corresponds, either with the predisposition of the individual to strumous disease, or still more, to the actual development of this state in the organ, and arrived at the probable conclusion that tubercles owe their immediate location to a mechanical process exceedingly analogous.

Waiving however, for the moment, all reference to the correctness of this inference, and having regard to the physical appearances alone, it follows that, if these be correctly stated, we should expect the separation of injected matter to be regulated by the increasing local tendency to deposition, and consequently find it exceedingly great in cases of decided and advanced phthisis.

The fact will be found to correspond strictly with the deduction, at least in all the cases in which I have made injections of the lungs: where the disease had run on to its advanced stages, I have always found, that the matter returned by the veins from such portions as remain respirable, has been deprived of its colouring matter to a large extent, while the capillaries themselves have exhibited that intensity of redness which results from a retention within their calibre.

But there are other, and equally important, circumstances revealed by examining the state of the pulmonary circulation when tubercles have actually occupied the lung's texture.

The respiratory process demands, for its due execution, two conditions—the free admission of air to the minute air structures, and the free circulation of black blood in the capillary vessels; if *either* fails the function is imperfectly performed—if *both* fail it is not executed at all on the parts where the impediment occurs. But the result, as regards the body generally, is different in the two cases: where the air cells and pulmonic capillaries are simultaneously obstructed, no arterializing action occurs in the portion affected, and the entire purposes of life are sustained, though somewhat imperfectly, by blood prepared on other portions of the lungs; but when, again, the air cells being obliterated, the capillary circulation still continues in parts of the lung, then not only is red blood formed in diminished quantity, but the quality of that produced is necessarily imperfect.

It is probable that the converse of this condition in no case exists: so long as the heart continues to act at all, its right side circulates blood throughout all those portions of the lung to which the air has access; but the first condition I conceive to occur partially in a very large majority of consumptive cases, and to produce evils to which the reader's attention will be subsequently directed.

The first physical change produced by pulmonary tubercle is obstruction of the air vesicles in minute points; as these increase in number, and approximate each other at the circumference, they obliterate the cell which was at first only partially occupied, leaving a thin layer of lung between each centre, till by a progressive action of this kind, a more or less considerable portion of the organ is rendered incapable of receiving air.

If in this state an injection be thrown in by the pulmonary artery, a considerable number of its minute branches are still found distributed over textures to which air no longer finds admission—a circumstance which seems to show, that the air vesicles or tubes become obliterated at a period anterior to that at which the vascular system in connexion with the right heart ceases to distribute blood from the centre to the circumference.

The appearances presented in cases of this kind are frequently such as might induce one to suppose that the tubercular matter itself had received the injection; and I have little doubt that where this has been stated to occur, and the vascularity of tubercle been consequently assumed, the error has arisen from too hastily deducing a conclusion from the general aspect of the preparation. A little attention will however, I think, place this matter in a different point of view, and show that the *substance* of the tubercle itself is in truth not possessed of any vascular apparatus, but that its *apparent* vessels are branches of the pulmonary artery, which still carry their contents in those thin layers of lung which intervene between each point by which the (so named) *tubercle* is constituted—and this even when the co-adaptation of these have not only succeeded in preventing entirely the access of air to the part, but formed distinct masses which are seemingly homogeneous.

Many portions of a phthisical lung are susceptible of having their capillary tissues pervaded by the injection, quite as minutely as if no disease existed in the part; those are such as still remain free from deposition, and on which, as it would appear, the supplemental breathing by which life is chiefly sustained, continues to be carried on; some portions again the injection does not penetrate at all, or in a very trifling degree, and this has seemed to occur more frequently in cases of acute phthisis, where a certain degree of inflammatory action has co-existed with the main disease, and induced partial hepatization.

In the third and more common variety, while the healthy portions remain permeable both by air and blood, those which are occupied by tubercle admit the latter fluid to some extent, while they deny ingress to the former, and the consequence is their pervasion by *black blood, which is not brought into contact with the air.*

Physical appearances of this kind are in some degree susceptible of description, but the idea they convey is more readily received by the eye, which bringing the points immediately before the reader, effects that, which much writing might fail to accomplish. I have consequently, in the second plate attached to this volume, sought to illustrate the physical phenomena now referred to: its figures are accurately copied from preparations in my possession, and they will be found to convey a very correct conception of the appearances generally presented by lungs under the circumstances of disease which they are intended to illustrate

The first figure of the plate represents the plane brought into view by the section of one lobe of the right lung after injection, in the case of a young woman who died of phthisis in its advanced stage.

The upper portion of the lobe was occupied by an extensive cavity, lined by adventitious membrane, and intersected by transverse bands of condensed pulmonary tissue, still traversed by open vessels of considerable size. The section is made across the lobe, about half an inch below the base of the cavity, intersecting the blood-vessels and air tubes in a transverse direction, thus exposing the cut canals of the larger arteries, veins, and bronchi. The fact so frequently adverted to, of the distinction in colour, between the contents of the two classes of blood-vessels, is here fully exemplified;—the larger veins presenting in their white sections a marked contrast to the deep red tint of their corresponding arteries, which contain the injection previous to its transfer through the capillaries. The main point however which the figure illustrates, is another condition of the pulmonic circulation, very generally present in the advanced stage of tubercular consumption.

Scattered over its surface are seen various groups of tubercles yet in their unsoftened state, whose close approximation, though sufficiently decided to prevent any access of air, has yet failed to obliterate completely the pulmonic vessels—thus showing in isolated portions of the lung, one, but *one* only, of the *two* conditions on which the reddening process depends. It will be observed that the still open vessels do not penetrate the substance of the tubercles, but ramify in the solid pulmonary matter which unites them, and the colour of the diseased portions is consequently far less decided than that of other parts where the lung still remains respirable and unoccupied by deposition: in one case the entire tissue has its capillary vessels minutely filled, in the other, its appearance is modified by the interspersion of grey tubercles in the midst of coloured vessels;—in the one case, the air and blood are brought into close approximation, as happens in a healthy lung—in the other, the latter fluid partially circulates, independent of the presence of the former.

The blacker specks, towards the centre of the plate, represent the cut extremities of bronchi of some size, on which I could desire to say a few words.

It appears singular why they should be particularly numerous and distinct in portions apparently more diseased than others, for it will be observed that where the injection has been most successful, and the tissue most perfect, they scarcely, if at all appear. In my experience this is the general rule, and the cause of such appearances seems explicable on the following principles:—in a lung entirely healthy—or in a lung which still presents healthy parts, the complete occupation of its vessels by the injected matter, so swells the tissue as to close the smaller air tubes by mechanical pressure; and hence, in such cases, a transverse section does not expose their open mouths; but when again the lung is partially consolidated, and the injection as a consequence does not so entirely occupy its substance, the mechanical cause which in the former case closes by lateral compression all but the larger bronchi, fails now to exert the same influence, and the result is, that the intersected cylinders of moderately sized air tubes occupy the surface of the section.

There appears to be also another reason why the minor bronchi, traversing portions of the lung partly solidified, are even more distinct and dilated than elsewhere. It is in phthisis a very common case, nay, the most common of all, to find obstructed portions of lung intervening between the grand inlet of air, and remoter parts, which still remain respirable: when in such a state of matters the thorax is dilated, the tendency evidently is to fill with air every portion of the lung still capable of receiving it; but the lower, and yet healthy portions, often require to be supplied by tubes which traverse diseased portions alone ; and hence it seems frequently to happen, that the constant current of air which flows along them, not only retains those tubes in a pervious state, but as before remarked, renders them even more distinct than usual. It would appear however, that they do not act efficiently towards the part itself, but simply as conduits, destined to supply others in which the air tissue still remains comparatively sound.

It would thus seem to be true as a matter of physical fact, that in many cases of phthisis, and as I conceive, especially in those which have run their course uncomplicated by inflammatory action, the pulmonic blood finds entrance into portions of the lung from which the air is excluded, and that consequently in such portions which doubtless vary in extent in different cases, those relations are subverted, on which, in a healthy condition, the due perfection of the reddening process depends.

A marked and striking contrast to this is found in lungs, which, having been the seat of inflammatory action, present some portion of their substance solidified by that lesion to which the name hepatization is applied. In such examples, instead of finding the right heart continuing to circulate blood over surfaces impermeable to air, the capillary vessels belonging to this system are entirely obliterated, being closed apparently by the same process which had produced the occupation of the air vessels, so that here, as a matter of physical fact, the pulmonic blood fails to be transmitted to textures unoccupied by air.

The 2nd Fig. of the second plate is intended to illustrate this point. The preparation was made from the lungs of a young subject, who having suffered severely from pneumonia succeeding measles, died after many months from marasmus, seemingly dependant on an impaired condition of the respiratory function, but without the lung exhibiting any traces of tubercular disease; different points in several of the lobes were no longer crepitous, and the injection was effected on the entire lung. Wherever the organ was solidified the injection failed to penetrate, while it readily pervaded all those portions which were still respirable. The plate represents a perpendicular section of one lobe, and it will be seen that the injection has ceased to occupy the vessels at a line very nearly transverse—a line separating the lower end of the lobe which was solidified, from the upper portions which retained their natural aspect.

Appearances similar to those now described, are frequently found in parallel cases, and I cannot therefore hesitate to conclude, that the pulmonic capillaries will be found generally closed in such parts of the lungs as have been solidified by inflammatory action.

It is here however necessary to distinguish between the true capillary circulation, and vessels of superior magnitude; for while the former, I believe, usually ceases to be carried on in parts so circumstanced, it is no unusual thing to find them pervaded by arteries of greater diameter. It would appear that this depends on the *situation* of the solid portion, and it is well known that this may be at almost any point throughout the organ.

When any portion of the lung continues respirable at a point further removed from the trunk of the pulmonary artery than the part affected, its larger branches pervade this to a greater or less extent* in their progress towards it, but do not divide into capillary vessels in the solid part itself, which consequently does not supply that other condition on which healthy respiration depends; these vessels in fact, like the smaller bronchi to which I recently alluded, execute the purpose of conduits only—conveying blood towards parts by which the breathing process is still carried on, but having no vital function to execute, till those have been reached. On the other hand, where the consolidated portion occupies the *extremity* of a lobe, and is consequently placed at a point further removed from the centre of circulation, even the larger and more appreciable arteries cease to pervade it—simply because there exists, neither in itself nor in more distant parts, any capillary circulation to be supplied.

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* The number or magnitude of these branches, it would appear, is determined by the extent of the healthy portion to which they lead; and this fact, as I believe is to be established by patho-

While one of these propositions is illustrated, as already shown by the 2nd fig. of the 2nd plate—the other receives similar confirmation from the 3rd fig. of the same.

In the case from which this preparation was derived, the person had been long liable to attacks of mild pneumonia, which had left behind it consolidated portions of the lung in different situations, but chiefly occupying the central parts of the lobes whose lower ends were yet respirable, and in fact,

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logy, may be urged in confirmation of the physiological principle insisted on early in the work—that variations in the quantity of blood received by parts are to be viewed as the *cause* not the *effect* of corresponding changes in the action of the heart. Vide p. 26—Et. Seq.

to some extent emphysematous. The injection was here also made on the whole lungs, and the figure represents a fair average section of one of the hepatized points which intervened between the trunk of the pulmonary artery and the lower end of the lobes; the result was, an entire and perfect pervasion of the minute vessels of the respirable extremities of the lung, while the solid portions above them contained only branches of some magnitude leading to those, but evidently serving no useful purpose towards such parts as in their passage they of necessity traversed.

I have thus endeavoured, first by description, and secondly by plates, to illustrate two positions:

1st. That where consolidation of the lung depends on tubercular deposition, there exists a physical condition, in certain portions of the organ, and certain stages of the disease, which permits the transmission of blood over surfaces to which the air is no longer admitted.

And 2ndly. That where consolidation of the lung depends on causes which are purely inflammatory, the pulmonary circulation ceases, on surfaces whose tissue the air has failed to traverse.

In all medical investigations it is much more easy to state physical facts, than to infer with precision the vital causes on which they depend; yet here a rational interpretation may perhaps be offered.

Throughout this whole volume I have, in common with many other writers, maintained that phthisis is not essentially an inflammatory disease, but this without attempting to deny that inflammatory symptoms, and the results of inflammatory action, are often, nay generally presented by cases of true consumption. We have consequently this great difficulty to contend with in attempting to investigate its primary and fundamental nature, that in its advanced stages, lesions essential, and merely adventitious, are so mixed up as to render their separation a matter of extreme difficulty. Assuming however the opinions already held to be correct, they, I think, lead us to something like a rational explanation of those causes on which the difference in the pulmonary circulation, in the examples recently quoted, depend.

That particular morbid condition of the blood-vessels, which we designate by the term "inflammation," excited by many causes—leading to many results—and referrible to all tissues, however obscure it may be as regards the precise functional changes involved in the action, seems at least connected with the capillary vessels of the aortic system. It is to some change in the working vessels of this class—or to some change in the blood on which, and by which, they work, that we must unavoidably refer the whole phenomena of inflammation, both in their origin and end; and although in the lungs where two systems of vessels circulate different kinds of blood, it is absolutely impossible to demonstrate the distinction between the actions they perform, yet analogically we may with reason conclude that inflammation of these organs, like inflammation in other parts, is in truth connected, not with the vessels of the smaller, but of the larger circulation; and that just as a remote part inflames, in consequence of some error in its aortic vessels, so do the lungs undergo the same process in connexion with an erroneous action of the vessels which sustain and nourish them as organs. Hence, in pneumonia, it is rational to conclude, on every analogy, that the bronchial arteries are those essentially involved, and the air vessels, and vessels of the right side of the heart, feel the effects of this morbid action just as they would be felt by any other tissues more remote from the centre; but feel them with greater intensity for a very simple physiological reason—that an erroneous action of the aortic vessels of the lung directly and immediately impedes the execution of that change in the blood on which, as organs, they depend, and on which again, in turn, the health of other organs rests; so that in fact, inflammation of the lung may, so to speak, be considered a suicidal act—the organ, poisoning as it were the very fountain, on whose equable and healthy flow its own vitality depends.

On these grounds I submit it as a probable inference, though not a demonstrable fact, that the cause of the closure of the pulmonic vessels, sometimes temporary, at others permanent, in a hepatized lung, is in truth similar to that which induces consolidation as the result of inflammatory action in remote organs which possess an aortic circulation only.

On this supposition, as the vessels of the pulmonic circulation are presumed to be, while health continues, dependant for their integrity of structure and capacity of action on vessels of the aortic system, so a disease peculiarly connected with an error of action in the latter vessels, affects them in a way analogous to that which occurs in tissues not strictly vascular, thus affording us a rational explanation of the physical fact before noticed, that in the hepatized portions of a lung the capillary circulation of the right heart is usually obliterated.

Again, a probable reason may on similar grounds be given for the continued permeability of certain of the minute pulmonary vessels when phthisis is yet in its early stage : its cause I have considered to be mechanical, not vital, in its nature—inducing indeed most important vital evils after it has acted, and remotely dependant on errors of vital action affecting the early stages of the nutritive function—but as it operates on the lung itself, strictly of the kind which I have named. On this view of the matter, it is not necessary that the aortic vessels should be at all involved in the actual production of the phthisical state; and it seems to me that many cases of chronic consumption come under our notice, in which the only wonder is, how the irritation of a matter foreign to the healthy textures should not have led to more decided local evils of an inflammatory character. When the pulmonic vessels are obliterated in consumptive cases of this non-inflammatory kind, their closure seems to be the result of pressure by the approximating tubercles themselves, and not to depend on any change in the organization of their coats produced by vital influences.

It is on this principle that we seem to arrive at a probable explanation of the fact, that in portions of the lung highly studded with unsoftened tubercles, we frequently find open pulmonary vessels, in considerable number, traversing and even ramifying in the solid structure. Of these vessels, some of greater magnitude appear to be trunks, conveying blood for capillary distribution to more distant portions of lung which are still respirable —just as often happens in the case of hepatization, while others—and this is the point of most importance—ramify on the solid texture, thus presenting a marked contrast to that entire absence of capillary circulation where inflammatory action has been the cause of solidity.

Whether this explanation of the causes to which we are to attribute the difference of vascular lesion in the two varieties of diseased lung described, be correct or not, is comparatively of very trivial importance; of far greater interest is it to trace the inferences, which unavoidably follow, as regards the influence which such physical condition must exert on the blood, that great agent in all functional or vital actions. To this part of my subject I now proceed, and of course do so on the assumption that, in the physical changes themselves I have not been mistaken.

I have on several occasions in the earlier pages of this book referred to a fact, which every man of experience and observation will probably admit, that the intensity of morbid re-action which arises from organic changes brought upon the lung, does not bear nearly so strict a relation to the *quantity* of the organ which is changed, as to the *nature* of the change itself.

It is probable that this principle might be susceptible of a more extensive application than it is in my power at present to attempt, and that of the many lesions to which the lungs are liable, each exerts an influence regulated quite as much by its character as by its extent; at present I must limit myself exclusively to the two conditions previously set forth—consolidation of portions of the organ resulting, first, from tubercular deposit, and secondly from inflammatory action.

Now in reflecting on the physiological reason, why either of those altered states of the pulmonic textures should induce a morbid re-action toward the body generally, it appears on a superfical view to be susceptible of an easy solution; the lungs are known to exert a most important influence on a fluid, which may properly be termed the *prime mover* in all vital change: and the rational conclusion therefore is, that as all the parts of an organic whole, exhibit a most admirable and wonderful adaptation to each other—so the lungs of a healthy and perfect animal possess sufficient capacity of action to meet, within certain limits, the demands of other parts. There is consequently no difficulty in arriving at a satisfactory explanation of why the body as a whole, suffers in consequence of impediments in its breathing apparatus.

But this explanation does not lie so completely on the surface, when we come to advert to the point—which observation as I conceive determines—that while a very *small* amount of organic change in the lung, under certain circumstances, induces a very *great* and persistent systemic re-action, so the converse of this is also, under different circumstances, equally true;—organic changes of considerable extent not only producing comparatively very slight re-action, but this also being only developed with any great force when, under the ever varying circumstances of a living animal, a large demand is made on the energies of the impaired organ. The causes originating this difference, I believe to be in some degree susceptible of recognition by a reference to the remarks on physical alteration already offered; which enable us to connect organic change with the functional evils which appear as its result. The leading point sought to be established by these observations is simply this—that in certain portions of the lung, consolidated by tubercles deposited in its textures, the air ceases to be admitted at a period when pulmonic blood still continues to be sent to them; there here therefore arises a condition of parts of much greater physiological importance than the mere abstraction of a certain portion of the lung from respiration; it is not that it fails to perform its function at all, but performs it in a manner hostile to the general interests of the body; for the blood which traverses these solid portions, must be returned in the same state in which it was when transmitted, and consequently in a condition unfit for the performance of any healthy action; in point of fact it seems a matter of certain inference, that under the circumstances named, the left ventricle receives a portion of black blood, which mingles there with other portions properly arterialized, derived from such parts of the lung as are still respirable; and that hence, the system is supplied through the aorta, with a degenerated fluid, whose power of sustaining vital action is impaired, in a direct ratio with the amount of that physical change in the lung of which such impairment is the consequence.

I am bound to admit that the proofs of this are founded on an induction only—though in my own belief a rational and consistent induction, from physical data furnished by morbid anatomy. I have had only two opportunities of examining blood derived from the arteries of consumptive patients, and neither presented any appreciable difference from that of healthy persons, in so far as the characters are concerned which distinguish the arterial from the venous.*

The obscure evidence however, derivable from direct observation, is not less felt in other similar questions than in this; the fact is, that chemistry has as yet failed to suggest any decided character beyond that of colour, by which the one variety of blood, in the same individual, can be distinguished from the other, and I need scarcely add that colour forms a criterion far too changeable and evanescent to demand much attention.

Independent of the well known fact, that the darkest blood very generally assumes the arterial tint when exposed to atmospheric oxygen after withdrawal, an instant mixture of the two varieties, even where the black predominates, assumes a character of colour approaching so nearly to the arterial, as to preclude us from saying with any certainty that the whole had not been the product of vessels connected with the left ventricle. Experiments of this kind are indeed necessarily made on blood exterior to the body, and it is certainly true that the impossibility of performing such without the access of oxygen, may more or less complicate this result; but I yet hold it a probable conclusion, that the aortic system under the circumstances already named, really circulates a mixed fluid, in some degree inadequate to the support of vital action, without the eye (our only present test) being able to detect the imperfection. He who founds his convictions in medical investigation, on physical evidence alone, must be content to remain a sceptic on more questions than the present.[*]

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* The difficulties attendant on an examination of the arterial blood in the human subject—for obvious reasons always great—are not diminished in phthisis; where, according at least to the author's experience, general bloodletting of any kind is seldom admissible. In the two examples referred to in the text, the occurrence of severe cerebral symptoms justified a section of the temporal artery.

That we are, as stated, destitute of proof derived from the colour of the blood, in support of the hypothesis maintained, ought to be received with one reservation. Those who have traced cases of pure uncomplicated phthisis to their termination, will agree, that it is by no means uncommon to find in the later stages, that dull, leaden, bloated hue—especially of the lips—which indicates a pervasion of the capillary vessels by a blood imperfectly arterialized. I have not however thought it right to insist on this as a special argument, because the same phenomenon is also frequently observed in other severe diseases of the lung altogether unconnected with the presence of tubercles.

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[*] The partial cessation of healthy sanguineous change, which is here stated occasionally to occur in the lung, in its relation to the system, sometimes also occurs, and that too in a more appreciable degree, in the system, in its relation to the lung; and thus, while arterial blood in some instances, is produced and transmitted of a description below the natural healthy standard, so also venous blood is occasionally returned of a nature more distinctly abnormal.

It forms no part of the objects of this book to do more than allude simply to the fact, but I believe that all men who have been in the habit of performing or observing many bleedings will agree, that it is not an unfrequent occurrence, to find the current emitted by a vein so red, as even sometimes to induce a dread, that an artery had been wounded. In such cases generally, the *entire* current is more red than natural; but in some—and I have more frequently remarked this in acute rheumatism—the vein sends forth a compound stream, one portion of which is dark, the other approaching to arterial, as

The unhealthy condition of the blood in consumption, which is thus inferentially concluded to occur, depends on the partial cessation of its arterial change, leading in a progressive manner to a depraved condition of the entire mass; the blood being thus rendered incapable of supplying in due perfection that stimulus on which perfect integrity of structure and vigour of action depend: for however probable it may be, that the material constitution of the blood is also altered by those errors of the nutritive process on which tubercles originally depend—still the morbid influence propagated by lungs occupied by those bodies, of which we now speak, would, without doubt, affect the vital properties of that fluid, whether its chemical constitution, or the mechanical arrangement of its particles, be in accordance with health, or the reverse. On this view, the direct morbid influence of a tuberculous lung, is not so much felt from any change wrought immediately on the matter of the blood, as on the temporary and transient qualities communicated to it by the act of breathing; and were we to assume the existence of an analogous pulmonic condition, induced by any cause differing from tubercle—where portions of the organ are still occupied by a capillary circulation, independent of the access of air—there

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if the trunk on which venesection is performed had received fluids differing in colour from two or more of its subordinate roots, which like the waters of the Rhone and Arve, as they leave the lake of Geneva, had not as yet become thoroughly blended.

seems little question, that similar physiological results would be produced. The fact however seems to be, that the same conditions do not exist in other diseases. In simple condensation of the lung, I have already endeavoured to show that the minute vessels and air cells are simultaneously obliterated—that both the conditions essential to respiration cease to exist on the part affected—and that hence, however great the difficulty may be of producing the quantity of red blood which the body demands, there is, connected with this, but little danger of the fluid, when produced, being of an imperfect kind.

Nor does bronchitis form an exception to the peculiar pathological influence which I have considered to be exerted by tubercle; for however marked be the analogy between the two diseases as regards their effects on the system, it only tends to show how the same result is sometimes induced by the operation of distinct causes. In acute bronchitis the air still traverses the breathing tissues of the lung, and the blood still pervades the pulmonic capillaries; the two conditions necessary to respiration exist, and the act is inefficiently performed only on account of the tenacious secretion, coating the surface on which their mutual action ought to be accomplished. If the disease be extensive and severe, the effect of this is shown in the most marked manner, not only by symptoms, but by the dark colour of the blood which the superficial capillaries contain; and unless this state be relieved by a removal of the cause which excites it, death by a process of asphyxia, speedily ensues. This I conceive to be the true pathology of acute bronchitis, when it involves large portions of the lung; we find it inducing a sudden deterioration of the blood sufficiently intense to strike at the source of all living action—a deterioration analogous to that in phthisis, but differing from it in extent, and originating in a distinct physical condition: the cause of death in one case, may be compared to the impetuous torrent which sweeps away a building—in the other, to the small but assiduous rivulet, which by a slow though certain process saps its foundations: life and the building are in the end equally subverted, and subverted by similar agencies, differing only in the speed and violence with which they act.

It is not however the extensive and acute form of bronchitis, but that which is limited in extent, and chronic in its character, which presents the strongest analogy to consumption, both in its symptoms and influence on the blood. Here, the lining membrane of certain portions of the lung become permanently thickened, while others remain comparatively healthy; according to the proportions which those bear to each other, the symptoms are severe or mild; so long as expectoration is easily accomplished—and so long as care is taken not to augment the demand for red blood by any unusual causes of excitement, the functions of life are performed with tolerable efficiency;

whereas, if action be increased, or if the secretion which coats the air surfaces be not constantly removed, or what is better, its production checked, the phenomena of the condition sustain a decided increase.

Cases of this kind frequently present, in all their symptoms, such a close similitude to true phthisis, that before attention was directed to the value of the physical signs, the two diseases were without question frequently confounded—and indeed often are so to this day, despite the improved means of diagnosis now happily possessed by the physician. The cause of this difficulty in diagnosis evidently depends on the close identity of symptoms attendant on distinct diseases, and these in either case result, first, from the immediate influence exerted by the pulmonary affections on the blood; and secondly, from the remote influence which blood so impaired exerts on the organs ; but the structural conditions which lead to this are different : in one case (bronchitis) air and blood continue to be brought into close vicinity—their mutual action only being impeded; in the other (phthisis), the air permanently fails to enter portions still traversed by pulmonic blood, and it is here not an impediment to action only, but the absence of one of the elements necessary to action, from which the general evils of the state spring.

But if there exists so striking a similitude in the morbid march of these two affections, the question may be reasonably asked, why phthisis is not equally susceptible of cure as bronchitis? For this there certainly exists *one,* and may be rationally presumed *two* reasons. The structural cause, which induces sanguineous imperfection, is less under the control of art in the one case than in the other; and further, the primary cause which originally leads to the location of tubercle, must in many examples be presumed still to operate and aggravate all the evils of reaction. It does not however follow from this, that consumption is necessarily a fatal disease, as it is too frequently considered; on the contrary, I hope to show in the sequel that the art of medicine, under the guidance of a rational pathology, may be yet enabled to grapple with its essential cause—not content with the vacillating uncertain practice, which unavoidably results from adopting symptoms only as its guide.

There remains one other disease to which it is necessary to allude in connexion with the special pathology of phthisis which I advocate. In introducing the subject I have said, that while in this malady pulmonic blood frequently circulates in portions of lung deprived of air, the converse of such condition never exists; but that where air enters, a provision is always made to continue the circulation connected with the right side of the heart.

To such a law, emphysema, without due consideration, might be fairly urged as an exception, for it is well known that in this disease, although the lung is preternaturally distended by air in some places, the blood has ceased, to a large extent, to pervade the vessels which are there distributed. A little reflection will however show that this constitutes an apparent rather than a real exception; the law I believe to apply with great strictness to all cases in which air enters and leaves the lung in that regular alternation which is essential to the act of breathing; and it was to this only that the observation was intended to apply.

In emphysema, on the contrary, such conditions no longer exist; instead of the air constantly changing, the very essence of the disease consists in the retention of this fluid in the dilated or ruptured cells; the fluid whose presence during a healthy state is necessary to respiration thus acting, during a diseased state, as a mechanical impediment to the function of the organ. The physiological results are consequently very analogous to those which follow on inflammatory consolidation of the lung; in both states the two conditions necessary to respiration fail on such parts as are involved by the respective diseases; there ensues on them an entire cessation of the function, life being in both cases supported by other and more healthy portions; but in neither does the physical change entitle us to infer, that the blood flowing in the arteries sustains an alteration in quality, however certain it may be that the difficulty of its production is augmented.

But if it be true that the morbid influence excited by a tuberculous lung towards the body, depends on an abnormal condition of the arterial blood, and if this state of the fluid is in turn the result of certain physical changes in the lung, so it would appear reasonable to conclude that the two states should be invariably connected, and that wherever tubercles existed in large numbers, they should manifest their presence by inducing distinct if not violent reaction.

Experience seems here to be adverse to any such conclusion, for we undoubtedly often possess very decided evidence of the existence of pulmonary tubercles, in persons who continue to enjoy comparative health for many years.

This objection, however specious it may appear, not only admits of an answer strictly conformable to the particular pathology of the disease, which I have maintained, but when attentively considered even goes some way in corroboration of its truth.

The leading anatomical fact on which this pathology rests, appears, as I have said, to determine that pulmonic blood continues in phthisis to circulate through portions of lung which no longer admit air; but though this state exists in many—and those the worst cases—it does not exist in all; neither when it occurs in one portion of a lung, does it necessarily occur in other portions of the same, even though they also contain tubercles. So long as those bodies remain isolated—or rather, so long as the minute specks, of which I have supposed an appreciable tubercle to be composed, merely unite into the formation of an apparently homogeneous nodule, healthy portions of lung intervene between them, on which, as on parts naturally circumstanced, the respiratory changes proceed with due regularity; and though I am far from holding that tubercles in this state offer no impediment to respiration, I still maintain that they only limit the function, by abstracting from it such portions of the organ as they immediately occupy, diminishing the surfaces on which red blood is produced, but not, as in other cases, interfering with the quality of its current; and hence, that they merely exert a pathological influence analogous to that which results from consolidation of the lung, originating in inflammatory action.

I have many times had an opportunity of examining the bodies of persons dying of other diseases, whose lungs contained large numbers of tubercles without chest symptoms of much intensity having existed during life. In such cases the isolation of the tubercles is always very marked, and the organ respirable up to their exterior edges; nor do they present that particular condition so often referred to, except partially in some examples where the progress of the malady had been marked by symptoms referrible to the chest.

The observations hitherto offered on this subject, relate to conditions in which tubercles remain in their primitive or unsoftened state, and apply more especially to the physiological causes of that morbid influence which is exercised towards the body by lungs so circumstanced. It must not however be forgotten that in the advanced stages of the disease, a considerable portion of the existing evils must be attributed to the process of softening—to the constant and large drain on the system which this involves, and the continued irritation which attends it—showing, in a very marked manner, how an action in itself established with a curative object, may, when proceeding to an undue length, invalidate its own intentions.

That the softening of tubercular masses in the lungs really depends on an effort made by living textures, having for its end the removal of a foreign matter which impedes their function, is now commonly admitted; and this effort would without doubt be more generally successful in the issue, were it not for two circumstances: first, that where the number of tubercles which simultaneously take on the action is large, the mere process itself is sufficient to exhaust the powers of life: and secondly, that in the greater number of examples, a perpetuity of softening becomes necessary, on account of the successive formation of new tubercles succeeding those which have been already extruded from portions of the organ affected at an earlier date.

But though those two adverse causes without doubt always oppose and frequently invalidate the efforts of nature altogether, the probability still seems to be, that within certain limits the system is capable of curing tubercles of the lung by resources inherent in itself, and the leading points of practice to which such a view obviously conducts, are two—the primary and by far most important being, a consideration of the means best calculated to arrest the act of *deposition:* and the second, a consideration of the means most influential in sustaining the powers of life under the act of *extrusion,* which, provided the first can be accomplished before any very large portion of the lungs is occupied, will not unfrequently be sufficient for itself. These are points however which more properly demand attention in the sequel; at present my object is to deal only with the connexion which exists between the softening of tubercles, and the peculiar pathology of phthisis in its earlier stages, which I have before endeavoured to elucidate.

In the earlier part of this volume I have endeavoured to establish that the capillaries belonging to the right heart, which are closed in hepatization, often remain pervious in such portions of lung as are solidified by tubercle; and I have there further attempted to explain the cause of this difference, founding on the principle that inflammatory action is present in the one case, but not necessarily in the other.

The same hypothesis applies with equal force to the late as well as the early stages of consumption. When softening commences in a portion of the lung extensively beset by tubercles, the process seems to be always attended by a certain amount of inflammation. On this truth the best of our modern pathologists are generally agreed. But while the morbid action thus set up, apparently with a curative intention, so alters the foreign matter as to facilitate its extrusion, it seems coincidently to exert an equally important influence on the condition of the pulmonic vessels, obliterating those which were open before its occurrence, and thus changing the lesion into the mere abstraction of a certain quantity of lung from executing the great physiological purpose which as an organ it fulfils. This appears to me always to constitute a part and portion of the process by which actual vomicæ of the lung are healed.

But in the contention which, so to speak, thus occurs between two descriptions of morbid action in themselves equally important, there exists every possible shade of difference in regard to the result which according to their respective preponderance is attained; sometimes the adventitious membrane which surrounds the cavity (in itself the product of inflammation) is encompassed by a portion of condensed lung presenting the phenomenon of circulation without air; sometimes the vessels of this part are obliterated, or to speak the same fact in different words, it is hepatized; in others the lung is respirable up to the exterior boundary of the cavity—while the vessels of some magnitude often found to traverse its thickened parietes, or to be included in the transverse partitions which run across its interior, may be ultimately traced in their capillary distribution to those healthy parts which as trunks they lead to. In other cases again inflammation proceeds in an exaggerated degree, and destroys the patient rapidly; the existing tubercles run into speedy softening—a copious evolution, not of their own substance alone, but of secretions poured forth from irritated surfaces in the vicinity, becomes established, and the patient dies; this appears to me to constitute the true solution of what has been popularly but expressively termed, "*galloping consumption.*" I have never after death examined a case of this kind without its presenting the most marked signs of previous inflammation united with tubercles—the latter probably constituted the fundamental disease—the immediate cause of death being the rapid progress of the former.

We are thus conducted to the probable conclusion, that a certain amount of inflammatory action, which is in my opinion essentially distinct from tuberculous action, is notwithstanding in very many cases necessary to its cure; and that hence, however requisite it may be to restrain this when it tends to proceed to an undue length, and thus to produce a disease more speedily fatal than the other, it is still necessary to avoid those measures which are calculated entirely to subdue it.

Now it is just here that the most difficult point in practice meets us: I have before said that art has little power in accomplishing, with any precision, what the efforts of the system very frequently effect; and for this reason, that though art can with great certainty stimulate any organ of the body into inflammation where a tendency exists, it cannot combine this with a limitation of the intensity or extent to which the action will proceed; and hence the difficulty here presented consists in appreciating the exact point at which medical means should be applied—on the one hand to increase, or on the other to diminish, an action at once curative and hazardous; curative in its object, but hazardous in its extension.

But though we thus cannot with precision always adapt our measures so as to produce a definite result, and although we are undoubtedly at times compelled to employ energetic means to subdue those higher degrees of inflammatory action which are connected with the tubercular state, yet I feel convinced that the view now taken of this question involves a principle of the highest importance in the general management of phthisis—that, namely, of exercising extreme caution in the employment of active depletion, unless urgently demanded by the rapid progress of a morbid action secondary in occurrence to the one which it is our leading object to remove. To discriminate the exact point at which active interference should commence, and the general sustaining plan of treatment be modified, is without question a difficulty demanding a considerable exercise of judgment, and a careful consideration of the entire circumstances connected with each case. To these points it will be necessary to advert more fully in the sequel; at present I would merely remark, that there is no more fallacious guide on which we can depend, than the velocity of the circulation. He who in phthisis trusts to a quick pulse as forming an indication for active depletory treatment, will assuredly often commit a very dangerous mistake: he who never employs it, on the principle that phthisis is a disease of direct debility, will not unfrequently be destined to lament, the accelerated march of a malady which, by a little exercise of discretion, might at least have been retarded, if not arrested in its course.

In concluding this chapter it may be desirable to take a rapid glance at the points which it has been my endeavour to establish in the preceding pages. I have considered tubercular consumption not as a simple but compound affection, essentially consisting of distinct stages, and distinct varieties of morbid action, some of which are referrible to the organ immediately involved, and some to distant parts, their morbid influences being reciprocally propagated through the medium of the blood.

First, I have considered as, very generally, a demonstrable fact, that the malady is primarily ushered in by manifest derangement in those earlier actions of the process of nutrition by which the heterogeneous matters of aliment are converted into chyle.

Secondly, that the immediate influence of this error is directed to the blood, whose venous current becomes charged with unhealthy particles which the lungs fail to identify with the common mass in a due and proper manner.

Thirdly, that though this state of the blood exists, to a greater or less extent, in all cases of strumous habit, it does not necessarily lead to the actual location of tubercle either in the lungs or system.

Fourthly, that this condition of the blood forms one of the elements necessary to the production of the local affection, but that this requires for its full establishment, the presence of a peculiar structural condition of a portion of the capillary vessels connected with one or other of the circulations.

Fifthly, that the location of tubercles owns a mechanical cause, and depends on a change in the natural healthy relation which the particles of blood bear to the tubes through which it flows; and that tubercles are hence constituted by innumerable minute points contained within the calibre of obstructed vessels.

Sixthly, having in this manner attempted to trace the successive steps by which tubercular disease of the lung becomes established, I have approached the question of these phenomena which appear to depend on the re-action of lungs so circumstanced towards the system, endeavouring inferentially to show, that those are mainly dependant on an imperfect condition of the arterial blood, necessarily resulting, in many instances, from the existing condition of portions of the lung.

Seventhly, that this condition depends on a continuance of the pulmonic circulation through parts of the organ to which air no longer finds admission.

Whether these conclusions are warranted by the observations and reasonings contained in the preceding pages, must be determined by others than the author. I shall only here add, that as they were gradually arrived at by a careful examination of the malady, both in its vital and physical relations, so they have also appeared to afford a somewhat substantial foundation, on which to rest principles of treatment, whether directed to its alleviation or cure. To this portion of the subject it is now my duty to proceed.

CHAPTER IV.

Treatment of Phthisis in its Stage of Formation.

OPINIONS DIFFER ON CURABILITY OF PHTHISIS—PRECAUTIONS NECESSARY IN DIAGNOSIS—PROPRIETY OF ATTENDING TO ITS VITAL AS WELL AS PHYSICAL RELATIONS—OPINION OF ITS CURABILITY—OBSERVATIONS ON TERM "CURE"—RESTRICTED USE OF IT—APPLICATION TO CONSUMPTION—STAGES IN ITS PATHOLOGY RECAPITULATED—INDICATIONS DEDUCIBLE—ERRORS OF ORGANIZATION NOT REMEDIABLE—OBJECTS OF TREATMENT CONFINED TO THE FUNCTIONAL CHANGES WHICH DETERMINE TUBERCLES—DISPEPSIA OF DR. TOD—ITS CONNEXION WITH PHTHISIS URGED—NOT ALONE SUFFICIENT TO EXPLAIN ITS PRODUCTION—SYMPTOMS OF STRUMOUS DISPEPSIA—MOST DANGEROUS WHERE A CONSUMPTIVE DIATHESIS ATTENDS—SIGNS OF THIS CONSTITUTION—TREATMENT OF STRUMOUS DISPEPSIA—MEANS OF PREVENTING ITS INFLUENCE ON THE LUNGS—PURE ALKALI, ITS USE AND THEORY OF ITS ACTION—PERSEVERANCE IN ITS EMPLOYMENT URGED—PRECAUTIONS—CONCLUSION.

Whether tubercular phthisis be under any circumstances susceptible of cure is a question on which some difference of opinion still prevails. That recovery frequently follows the full development of all those symptoms by which tubercular disease of the lung is usually characterized, admits indeed of no doubt; but as all these may certainly result from pathological conditions of the organ, in which tubercles have no share, so it follows, that where the existence of phthisis has been inferred from symptoms only, the probability is nearly equal, where their subsidence has ensued, that an erroneous diagnosis had been given, as that tubercular degeneration has in reality been subdued.

Before therefore the claims of any plan of treatment, apparently beneficial, can be consistently admitted, it behoves the inquirer to satisfy himself, by the most rigid examination, that tubercles had truly an existence in the cases which have come under his review, and consequently to employ all the diagnostic means available, in the attainment of such end.

Though fully alive to the great difficulties we have to encounter in this particular, I still hold, that a close attention to the history, progress, and existing aspect of the case—a cautious review of its symptoms—and above all, perhaps, a sedulous employment of those physical methods of inquiry taught us by Auenbrugger and Laennec, will at least very generally so illustrate and aid each other, that the greatest certainty may be attained, and if after the existence of tubercles has been indicated by such means, in a large variety of cases which have been apparently benefitted or cured by the adoption of a particular practice, I conceive that we are as fully justified in assuming the fact of its efficiency, as we are in assuming the efficiency of remedies applied to any other variety of disease.

It is not my intention here, to enter on any critical examination of the various diagnostic means applicable to consumption, already so amply discussed in numerous other treatises the offspring of Laennec's original production, conceiving as I fully do, that the present position of the medical art demands a greater attention to the vital relations of phthisis, and the rationale of its treatment founded on this, than is usually bestowed—the physical signs which indicate its presence having as I fear too exclusively absorbed the attention of inquirers. To ascertain with accuracy the existence of a disease is, without doubt, of immense importance, and forms no small advance towards its removal; but the utmost precision here is of minor utility, if it only leads us to predict the inevitable fate of the patient; dare I add that it may be worse than useless, should it induce that hopelessness of mind which unavoidably leads to apathy, and tends to paralyze every attempt to advance one department of the science which we cultivate?

I have for these reasons contented myself with preceding the detail of cases which conclude this volume, with a brief announcement of the general points of evidence on which I have relied, each individual case containing also, in the report, a notice of the special signs, symptoms, and collateral history presented by itself. This plan, it appears to me, offers to the reader a fairer opportunity of estimating the exact nature of the cases, than could possibly be attained by an abstract discussion on the general question of diagnosis.

Passing over, therefore, for the present, any further allusion to the means by which consumption is most surely indicated, and assuming that the evidence of its existence in the cases subsequently stated was tolerably perfect, I shall proceed at once to consider the methods of treatment employed, and the principles on which this rests as directly connected with the general pathology of the disease set forth in the preceding pages; and I do so with the preliminary statement of my conviction, founded on no very limited experience, that as a matter of fact, phthisis is not only frequently arrested in its progress, or remedied after full development by some obscure sanative effort of the system, but that, besides this, it is a disease amenable to the control of remedial means when these are applied in a proper manner, and directed under the guidance of a rational pathology.

In medicine, as in more abstruse inquiries, many difficulties originate in the vague use of words; and of all the terms employed in medical writing, the word "cure" is perhaps the one which stands in most need of definition. In its broadest sense, as usually employed, it appears to express, the *complete* restoration of a part, or of the whole body, which had previously erred either in function or structure, to a normal state; and this, without its necessarily implying any connexion between appreciable cause and effect, inasmuch, as such restoration is frequently induced without our being capable of explaining it. But again, the term is commonly, and as regards the medical art, more properly used, to express a direct connexion between the means producing, and the effect produced—to indicate not only the fact of restoration, but the agencies, be they medicinal or dietetic, on which the change is presumed to depend; and it is in this sense that I mean to employ it in the succeeding pages. To say that phthisis is at times cured by an effort of nature, and to show the structural or vital changes through which this result is attained, constitutes without question a most interesting subject of pathological research; but practical medicine seems to require that we should proceed a step further, and, by attempting to connect the means with the end, endeavour to accomplish by the aid of art, what we admit to be at times effected without such intervention.

But while we thus define the term cure as in strictness implying the *entire* restoration of function and organization, there exists a more modified sense in which it may be, and often is, employed. Perfect integrity of organs, or perfect integrity of action in these, is found it may safely be averred in very few human beings after life has a little advanced, and it hence follows that the term may be legitimately used to express not indeed *complete* restoration, but only such an approach to this, as enables the individual, under certain limitations and modes of management, to carry on life without inconvenience, even to a protracted period. This applies with peculiar force to all that large class of diseases which depend on altered structure; for it seems exceedingly questionable whether organs affected in their material constitution, are ever susceptible of complete repair, while, on the other hand, it is certain that function is often performed in a tolerably efficient manner by parts not structurally perfect; but as the demands made on the functional duties of all organs are constantly varying in degree according to the operation of a thousand causes, so it may be set down as a law, liable to no exception, that parts affected in their structure are never capable of executing properly those higher degrees of action which they effect with great facility during their healthy state; and hence it happens that persons known to carry about with them certain organs in a diseased condition, enjoy an existence of tolerable comfort till one or other of these is called on for an unwonted exercise of its powers. To no organs do these remarks more strictly apply than to the lungs: on account doubtless of their extreme importance in the economy of life, nature has provided so ample a volume of the breathing textures, that they are fully exerted only under circumstances of unusual action, and the consequence is, that a certain amount of this volume may be permanently abstracted without any marked effects ensuing, excepting under circumstances of high excitation. Now as, according to the present state of our knowledge it seems probable, that tubercles when once established in the lungs are, as a general rule, unsusceptible of removal by absorption or otherwise in an early stage, and as their expulsion by the act of softening necessarily involves a destruction of the tissues in which they immediately have their seat, so the conclusion follows, that a perfect cure of phthisis, if we express by this an entire restoration of the organ, can seldom if ever be accomplished ; but if we restrict our definition of this term within the limits before stated, and admit that a portion of the lung may be spared without any compromise of life, then I hope to show that we are in the possession of means frequently capable of arresting in its course the progressive deposition of tubercular matter, and thus restoring our patient to a state of comparative health, although deprived of a certain portion of the organ immediately involved.

The pathology of consumption, already sufficiently explained, recognises three stages :—

First, A primary stage, connected with erroneous action of the digestive organs, in consequence of which the matter of tubercle is presumed to be produced.

Secondly, A deteriorated state of the blood induced by this, during the continuance of which, that fluid presents one of the conditions necessary to the formation of tubercles, their location being contingent on a peculiar pulmonic organization, whose presence is therefore essential to a full production of the disease.

Thirdly, A stage of re-action towards the entire organs and functions of the body, as a direct result of tubercles after their location, the intensity of this being dependant not on their amount alone, but also on the peculiar influence which they frequently exert on the pulmonic circulation.

The indications of cure, which arise as direct corollaries out of these positions, are consequently three also:—

First, To counteract that morbid state of the digestive organs originating the matter, without which tubercles cannot be produced.

Secondly, To accomplish a solution of this matter, after it has passed into the blood, and thus arrest its local deposition, presumed to depend on mechanical retention in the extreme vessels of the pulmonary artery.

Thirdly, To place and retain the patient under such circumstances, in reference to his medical, dietetic, and general treatment, as seem on rational principles best calculated to meet those evils which result, from the existing state of his respiratory organs, thus affording time for removal by the natural process of softening of such tubercles as already actually exist.

In arriving at these general indications, under which, the details of treatment may be conveniently arranged, I have omitted to take into account those peculiarities of organization which appear to determine the local production of strumous deposition. How far such structural conditions may result from the action of various causes affecting the individual, and how far they are dependant on transmission downwards, from parent to child, it is no easy matter to determine; but it at least appears improbable that, being present, they should be remediable by any means applied to the individual; and it hence follows that the immediate power which art possesses over the formation of local struma, is limited to the counteraction of those functional errors, and of that morbid condition of the blood arising out of them, which cooperate with a capillary organization fundamentally imperfect, in its full establishment.

Further, although the two first indications refer to separate stages in the pathology of one disease, yet in practice we are unavoidably compelled to consider them together; the two processes are probably all but simultaneous in their occurrence, or at all events we cannot discriminate the symptoms peculiar to each, and are hence obliged to examine at the same time those measures of treatment best calculated to meet the morbid changes going on, whether they be supposed to act by suspending the formation of tubercular matter, or preventing its location after this has taken place.

I shall therefore in this chapter submit the results of my experience, as regards the practice best calculated to fulfil those conjoint indications.

Though the majority of writers on consumption have, in terms more or less general, alluded to those disorders of the digestive organs by which it is so frequently attended, yet the precise connexion between those two conditions of a common malady, had till of late years been but vaguely stated. The article on Dispepsia, by Dr. Tod, contained in the Cyclopædia of Practical Medicine, was perhaps the first treatise which especially directed attention to the subject, and the Profession are, without question, much indebted to the author for pointing out, under the name of "Strumous Dispepsia," the characters of that abdominal derangement, which constitutes so constant and important an element in the production of local struma, as marked by deposition. Sir James Clarke, in his valuable Treatise, has applied these views in a special manner to tubercles of the lung, and it may now in fact be considered a very generally received opinion, that errors of action in the organs destined to elaborate chyle, have a large share in the production of that peculiar constitutional condition which he emphatically names "tuberculous cachexia."

In the general opinions of these authors I entirely concur, but at the same time submit, that they are not in themselves sufficient to account for the local phenomena of strumous affections; otherwise it would follow, that the strumous dispepsia of one author, or the tubercular cachexia of the other, could not exist, without being followed or accompanied by tubercular deposition in some part of the frame. Experience, I am convinced, proves this, not to be the case; but that both conditions may exist—nay, exist in some instances in such intensity as to produce a fatal issue, without anatomy offering evidence of the presence of tubercle in any organ whatsoever. The case in fact seems to stand thus, that while the location of tubercular matter never occurs independent of dispepsia more or less distinctly marked, yet that this latter affection, evidently of the strumous form, may be present without tubercles resulting, and there is consequently something wanting to connect their local development with that anterior disorder of the digestive organs which would appear to originate the matter of which they are finally constituted. It is this link which I am not without hope may be in some degree supplied by the pathology of consumption contained in this work. But whether this should prove the case or not, one thing is certain, that a close attention to those earlier disorders of function, in the organs which elaborate chyle, is of the most vital importance in the treatment of phthisis—that an acquaintance with their symptoms is indispensable to any rational practice—and that their removal, before ulterior effects are manifested by the lung, offers the best prospect of successfully combating the disease.

The form of dispepsia here referred to is mainly characterized by two sets of symptoms: those which announce considerable irritation of the mucous membrane of the bowels,—and those which indicate an unhealthy condition of the abdominal secretions, more especially, of the liver. The papillæ of the tongue are red and prominent, frequently, as stated by Sir J. Clarke, projecting through a dirty whitish fur which chiefly occupies its back and central surface, leaving the edges tolerably clean but of a brighter red than that of health; as a general rule the organ is moist, flabby, and tremulous, but when the disease exists in great intensity, it is often dry and glazed. The tendency of the bowels is towards costiveness, and evacuations when procured, though variable in colour and consistence, are never of a healthy character; the matter extruded is mixed with mucous or gelatinous discharges, sometimes tinged with blood, and various depraved secretions which render the stools frothy, green, and fœtid; on other occasions the motions are of a colour considerably lighter than that of health, from an apparent suspension of the biliary secretion, while on others the liver acts with an augmented activity, of which a bilious diarrhœa is the result.

During this state of things, the abdomen is, almost constantly, tumid and drummy, the appetite capricious, though generally keen and craving, but nutrition languishes almost in proportion to the quantity of food which is received, and the lower extremities especially become weak and emaciated, sometimes appearing like slender dangling appendages to an inordinately bulky trunk.

These are one and all symptoms extremely analogous to those which mark what has been often named the infantile remittent fever, and may, or may not, lead to the establishment of local struma, according to the presence or absence of other determining circumstances.

The condition which they announce may become engendered, we have reason to think, even in a body of the most healthy structure, from various causes, such as bad or deficient food, impure air, and the like, especially during the earlier periods of life, but it is certainly more commonly found and more readily excited in those persons, stamped by what has been termed, a phthisical diathesis—a form of constitution undoubtedly transmitted downwards from parent to child, and often found to pervade the members of a whole family through many generations.

The signs by which the presence of this constitution is marked, have been so well and frequently described from the days of Hippocrates downwards, that it is here only necessary to allude to them with great brevity. They are such as indicate, not only a peculiar organization, but a peculiar habitude of vital action as connected with it. More commonly the skin is delicate, clear, and waxy, and the hair light; the contour of the face, body, and limbs often elegant, and the symmetry of the person good; but the muscular system is notwithstanding flabby, and incapable of powerful action; the upper lip is usually tumid, the eyes large and lustrous, and their sclerotic, of a pearly whiteness; the teeth are said to be unusually sound and regular, a remark, which if not universally, is at least often true. The ratio which the venous system bears to the arterial has been stated to be abnormally great, and certain it is, that the former vessels appear to be more numerous and large than usual; how far this may be deceptive—dependant on the transparent delicacy of the skin, I am unable to determine.

The actions of the vascular system are marked by some peculiarities; the power of the heart and force of the vessels is under the standard of health, and a weakness of the whole circulation prevails; the extremities are consequently often cold, liable to œdematous swellings, and at times affected by chilly partial sweats; an inaptitude to muscular exertion prevails, correspondent to the general weakness of the circulating organs, and the patient exhibits a languor and listlessness of body never found in a well constructed frame while youth remains. But though the power of the vascular system is small, its excitability is great, and as the intellectual powers and functions of the nervous system are nearly in all examples highly developed, so slight causes of excitement, whether mental or corporeal, produce effects on the circulation which are not found in a body naturally robust. Hence arise the sudden flushes of the countenance from trivial causes of mental emotion, which frequently suffuse the cheek of beauty with a blush originating in a fatal tendency, and hence the sudden but ill sustained fits of transient vigour, very foreign to the nature of the person who exhibits them. It is in constitutions presenting these peculiarities, closely connected with a tendency to deposit tubercles, that strumous dispepsia, the necessary precursor of their formation, is by far the most dangerous; the entire of its symptoms may, in my belief, be present in persons who do not exhibit any decided marks of a strumous habit, but it is on the former that it tells with peculiar force, and in which, of all others, there exists the greatest necessity for combating, on its very first appearance, any deviation from health in the functions of the abdominal viscera.

In our endeavours to effect this object, it must be held in view, that these conditions form slowly and progress insidiously; that they often occur in organization defective from the beginning, and that medical attention is seldom directed to them till some advance is made; cases of the kind are therefore rarely objects of active treatment, but the means best adapted to their cure are such as correspond, in their slow gradual action, with the chronic character of the affection itself. I throw out this general remark from feeling convinced, that an error of no slight importance often arises from an overweening confidence in the efficacy of medical resources, an error scarcely less injurious to the interests of those committing it, than that absurd scepticism on the same subject which occupies the minds of some. In consequence of this feeling, people are frequently too apt to expect, during the existence of chronic maladies, that the influence of remedial means should be shown in a manner more speedy and decided than, unfortunately, is ever likely to be the case; it is no easy matter to cause them to view this point in its proper aspect, and they are either anxious to urge on the physician to some vigorous line of practice, which if adopted is often productive of much evil, or else to withdraw from him confidence, which I grant it is difficult to retain, in examples where success is usually dependant on the continued application of measures, mild and gentle in character, and which prove their efficacy more by final results than speedy amelioration.

A most important part of the treatment in that form of dispepsia which precedes the development of consumption, consists in a proper regulation of the diet. The whole phenomena of the disease warrant the conclusion, that, combined generally with a strong tendency to receive nutriment into the stomach, there exists also a morbid incapacity of digesting it, after reception ; one leading object therefore in the choice of diet is, not simply the introduction of matter containing a large amount of nourishment, towards which from the attendant debility there exists a strong temptation, but of such as, under existing circumstances, the organs of digestion are best able to prepare; for it may be laid down as a general axiom, applicable to every form of dispepsia, that the support or renovation of the body depends, not on the amount or quality of food received, so much as on the power which they possess of elaborating this in a proper manner. Unless this be attended to, the injesta, however highly charged with nutritious elements, become only, causes of irritation, which aggravate the debility they were intended to remove.

There are therefore two points of great importance in treating the disease more immediately under review; first, that the substances employed for the purposes of nutrition should be of such a kind as the digestive organs are able to concoct; and secondly, that those should be presented to them in such quantities only, and at such intervals, as best conduce to this end.

In endeavouring to effect the first of these intentions it must be held in remembrance, that the lining membrane of the stomach and bowels, on which all the injesta act mechanically, is in a condition of morbid excitement, and peculiarly disposed to have this increased by slight causes which in the healthy state would be inoccuous, and that this, while it weakens the power of digestion, increases at the same time the appetite for food. We are thus as it were called on to reconcile, as may best be done, two contradictory conditions—to supply nourishment to a weakened frame through inlets occupied by disease of a nature which forbids the employment of dietetic articles, containing the elements of nutrition in a concentrated form.

To meet these difficulties in an effectual manner is not always an easy task, but experience appears to determine that this is best accomplished by the employment of a mild farinaceous diet, which possesses the advantage of being converted into chyle, without producing that irritation of the digestive organs which more highly animalized articles but too generally excite. In milk we have presented to us an intermediate fluid, which from the very earliest ages has been extolled in the treatment of consumption in all its forms, and is not less adapted to its preliminary stage of which we now treat. There are very few cases in which it will not agree with the stomach, and should, in combination with the farinaceous food already named, form a large part of the diet employed.

Of the various kinds of milk, that of the cow is, in this country at least, what is generally used; that of the mare, goat, sheep, and ass, have however been respectively extolled as much superior to the other; I am not prepared from experience to deliver any opinion on their advantages, except as regards the last. It contains less curd and a larger share of whey than cows' milk, is consequently of a lighter quality, and in a few examples, where that cannot be taken with comfort, offers a very valuable resource; but unless in such examples, it has not appeared to possess advantages commensurate with the expense and inconvenience connected with its use. In the employment of animal food we must be mainly regulated by the attendant circumstances of each special case. When the irritation of the mucous membrane is considerable, this, in its solid form, is altogether improper, and should never be attempted till the symptoms are diminished. When again the debility is great, and the abdominal irritation not very marked, it is often proper to employ in part an animal diet in solution.

In the preparation of soups or broths, there is one important point to be attended to—the avoidance of all fatty matter in their composition ; they should therefore always be allowed to cool before use, by which precaution any grease which they contain is effectually separated. Beef, mutton, veal, and chicken, are the materials of which these solutions are usually made. In real utility they stand much on the same ground, but as gratification of the taste, in itself, acts as a specific stimulus to digestion, it is proper to consult the wishes of the patient as to the particular article he desires.

Among other symptoms attendant on the form of dispepsia under consideration, I have already alluded to the frequent presence of an appetite morbidly craving; and having thus to deal with a strong desire to receive nourishment on the one hand, and a great incapacity to digest it properly on the other, it becomes a matter of considerable importance that, having selected a proper description of food, it should be taken in such quantities only, and at such intervals, as best ensure its due elaboration.

It must be confessed that this requires some resolution on the part of the patient, and some firmness on that of the attendant. As the disease is more frequent in the young, we have not generally the advantage of enlisting reason on our side; nor is this a matter of much astonishment when we reflect, how seldom, even in mature years, the appetite is under the control of judgment. On the other hand there appears to attendants something cruel in restraining the expressed desires of the invalid; it is not easy to impress their minds with the vast difference between the healthy desire for food, and that depraved state of the appetite which has its origin in disease; and thus, though it be very common to see emaciation proceed in a direct ratio with an injudicious augmentation of the diet, the cause of this is too frequently overlooked, and the disease continues to receive fresh aggravations from the improper indulgence of a feeling engendered by itself. It is however the duty of the physician to combat popular errors of this kind, which while he admits them to be natural, he knows to be hostile to the interests of the sick, and I know of few that demand an expression of his opinion more than the one here noticed. But it is more easy to enunciate the general proposition, that appetite is not always to be viewed as a just criterion of the nourishment required, than to lay down a precise law as regards either the time, or quantity, of administration; for this must in a great degree depend on the whole combination of symptoms present in each particular case. The best general rule may perhaps be found, in never supplying at one time so large a quantity as the appetite indicates, and repeating this again within a time considerably less than is usually allowed to intervene between the meals. If the first point be strictly attended to, the invalid should never exceed, at most, three hours without a renewed supply. The great practical difficulty here to be encountered consists in this, that the patient may, so far as his feelings are concerned, be said to exist constantly in a state of partial hunger, but even this is better, and attended with less of suffering, than that uneasy febrile irritation which springs from a stomach loaded with contents it is unable to dispose of; independent of this, the resolution required very soon creates its own reward, for as the disease moderates under this and other proper measures, the craving diminishes with the removal of the conditions out of which it sprung, and the lacteals, supplied with a better prepared and more nutritious chyle, perform their functions with an energy to which they were before unaccustomed.

But while a large and most important portion of the treatment must thus always depend on a proper regulation of the diet, measures strictly medical are by no means of minor importance. We have morbid secretions to correct, local irritation to subdue, and new tone and vigour to communicate to the whole digestive organs; above all, where the phthisical tendency is strongly developed, we have if possible to counteract that morbid condition of the blood, which springing immediately out of the dispepsia itself, constitutes, as before stated, an intermediate link in the complicated chain of causes and effects, which connects the primary formation of tubercular matter in the digestive organs, with its final deposition in the lungs.

Some of the most common and prominent symptoms of the affection indicate, with great distinctness, imperfections in the action of the liver; the white or clay-coloured evacuations which so frequently occur, point not with less certainty to a temporary suspension of its functions, than do the dark fœtid discharges often met with, announce that these are executed in an erroneous manner. Now though we are by no means as yet fully informed as to the purposes which the liver serves in the process of digestion, we are at least aware that they are very important, and as we still further know that the whole of the abdominal venous blood which conveys to the lungs, the new, and as yet unassimilated, elements of nutrition, necessarily passes through its substance—so in a malady dependant on a morbid condition of the blood for its local production, there can remain little question that functional errors of the liver are highly influential in its establishment. There are hence two distinct modes in which derangement of this viscus may be fairly presumed to influence consumption—first, by preventing a due elaboration of the nutriment in the act of digestion, and secondly by causing it to fail in producing on the abdominal blood, during its return to the lungs, those obscure changes which we have every reason to believe it does perform when health remains.

The medicines which exert the greatest influence in correcting these morbid states, are those to which the name of alterative has been usually applied, and among such, mercury when judiciously administered is without doubt the most important. Of the many preparations of this metal, its chloride or calomel, and the protoxide, either under the form of blue pill, or forming by mechanical mixture with chalk, the blue powder of the Pharmacopœia, have in this country been chiefly used; and in selecting from among them, I am disposed to give a decided preference to the two last. In the employment of calomel, where irritation of the mucous membrane exists, this important difficulty occurs, that while it without doubt acts with great energy on the liver, it is too apt to augment the membranous disease; and I feel convinced that in practice one often sees it so unsparingly employed, and continued so long, that the remedy aggravates and keeps up the very symptoms it is intended to subdue. It ought on this account to be always used with great circumspection, its effects carefully noted, and the remedy suspended when any signs of increased irritation show themselves. When using it in my own practice I have seldom exceeded the alterative dose of from one to two grains, given about each alternate night, and even then have usually deemed it prudent to combine the remedy with small doses of some anodyne, such as Dover's powder, or the extracts of henbane, or conium. Of the extremely minute doses in which it has been urgently recommended by Dr. Wilson Philip, my experience is not sufficient to justify a decided opinion; though there are doubtless many diseases of the liver in which it may thus be given with both safety and advantage. It is however seldom necessary to encounter the risks more or less connected with the use of calomel, for the benefits expected from it are equally derivable from the milder preparations of mercury. Of these the *hydrargerum c creta* of our Pharmacopœia is the one which, especially in young persons, answers generally the best, and may readily be combined with such other remedies as the exigencies of the case demand. Of all these combinations I know of none which so well fulfils the duties of a mild alterative as this, in combination with rhubarb, to which a grain or two of ground ginger or aromatic powder may be added. The doses in which it answers best, are small ones, repeated each or every alternate night, and continued till the colour of the evacuations, the appearance of the tongue, and diminished tumidity of the abdomen shall have indicated an improved condition of the digestive organs. The quantity employed must be in some degree regulated by the age of the patient; from three to five grains of the mercurial powder, united with from four to six of rhubarb, and one or two of the carminative powder, may be considered as about the proportions required for young persons under twelve or fourteen; nor is it necessary much to augment the quantity in those of greater age, for as the remedy is one purely alterative, and therefore not intended to produce any marked sensible effects, and as children bear the continued use of mercury proportionally better than adults, a very slight increase of the dose is all which is required when its slow progressive action is desired.

When cough, and irritation of the lungs prevail at this early period, without speculating on whether the symptoms arise from sympathy with the digestive apparatus, or from causes directly connected with the respiratory organs themselves, ipecacuanha forms a valuable addition to the alterative remedies already named. It appears to exert a specific influence over the abdominal as well as pulmonary secretions, and frequently to aid mercury in effecting their improvement. Of its uses in the advanced periods of consumption, where there no longer remains a doubt that the symptoms essentially depend on a reflected action of the lungs, I shall have occasion to speak more in a future chapter.

Inaction of the bowels being a very unusual symptom, its removal becomes a point of great moment, for the depraved secretions when long retained act as new sources of irritation, aggravating the functional errors which produced them. In effecting this purpose, one principle must be kept steadily in view—that it should be attained with the least possible irritation; for if it be true that a bowel, whose lining membrane is in a congested state, be peculiarly liable to have this increased by the continued presence of acrid contents, it is not the less certain that even greater disadvantages may arise from the improper use of active purgatives, directed to their removal. It not unfrequently happens that the alterative plan of treatment before advised, is of itself sufficient, when aided by a proper diet, to retain the bowels in a state sufficiently open: when this is not the case, in selecting additional means, I am not acquainted with any remedy which attains the end with greater efficiency and safety than castor oil, given in small morning doses, and at such intervals as the circumstances of the case demand, limiting the quantity to that which is merely sufficient to evacuate without purging; when griping or nausea attends its use, the addition of a few drops of laudanum, or the tincture of hyoscyamus, renders its action more easy and effective. When castor oil from any cause cannot conveniently be taken, a solution of manna, either in water or milk, or if a stronger action is required dissolved in small doses of the infusion of rhubarb, offer valuable substitutes. The saline purgatives appear to me highly objectionable; they frequently irritate instead of soothing the bowels, usually fail in effecting the intentions for which they are given, and even where they seem to accomplish this, the discharges are of that serous character which exhaust the fluids, without removing those morbid accumulations which it is our leading object to carry off. It would be attended with little advantage to go through the long catalogue of aperients which our Pharmacopœias so liberally supply; the entire principles which should regulate our conduct in their use is of more importance, and may be summed up in a few words, that active purgation is always injudicious, that even a mild action on the bowels artificially induced, ought to be adopted with caution, accomplished by the gentlest available means, and with the least possible irritation.

The condition of the mucous tissue, though not strictly one of inflammation, occupies when considerable the almost undefinable line between simple excitement and inflammatory action, and is at least characterized by congestion of the vessels. In combating this, however, it is seldom necessary to have recourse to the abstraction of blood, for this state being mainly dependant on obstruction in the liver on the one hand, and extreme susceptibility of the membrane on the other, it better yields to other means, without impelling the judicious practitioner to adopt measures, which, even if strongly demanded, he would employ with a trembling hand on account of the general debility which prevails. The same objection however does not apply to counter irritation practised over the abdomen, which, at the same time that it moderately stimulates the cutaneous vessels, is of no small use from the mechanical influence it exerts on the viscera contained within the cavity.

In effecting this it is wise to avoid those applitions which either stimulate at the time greatly, or by producing vesication keep up continued irritation; for the nervous susceptibility is usually great in such cases, and what might be gained on the principle of revulsion is more than lost by a continually exaggerated condition of the sensitive function; blisters, and all means by which a breach of continuity is produced, on these grounds appear objectionable; while considerable benefits result from the use of warm liniments perseveringly employed. The exact composition of these is perhaps of less consequence than a recognition of the principle which dictates their use; the soap liniment of the shops answers every purpose as a basis, but its strength is by no means great, and this often requires to be increased by the addition of other ingredients in such proportions as may accord with the irritability of the skin in particular cases; by adding to each ounce, from one to two drachms of the liquor of ammonia, or from one-half to one drachm of the tincture of cantharides, we attain with a little management the object required. In the execution of this intention, the liniment should be applied by gentle friction over the whole abdomen, in the morning and evening of each day, for about ten minutes or a quarter of an hour, using the precaution of suspending such practice for a day or two should any excessive irritation be produced. Oleaginous or fatty applications appear less proper in principle, and certainly less convenient in practice; it is of great importance to preserve the skin in a transpirable state, with which ointments in some measure interfere, and hence careful ablution ought always to be employed afterwards, when any such are used; as regards the superior convenience or comfort of the liniment it is probable that little difference of opinion can prevail.

There exists a greater necessity for attending to the removal of all mechanical impediments to a due execution of the skin's function, because, as we shall yet have occasion to see, it in all probability becomes a substitute to some extent for the lungs, in the advanced stages of consumption, and hence though it be by no means certain that phthisis will supervene on the affection now discussed, it is at least prudent not to adopt any measures which may impede the cutaneous function, where such an event may be reasonably anticipated.

For a similar reason the warm bath should be employed once or twice a week, and the body not merely immersed in it, but care taken to cleanse the whole surface effectually by the assiduous use of the brush.

On the efficacy of those medicines which are usually classed under the name of tonics, much difference of opinion exists. It must be admitted that from the signs of general debility which attend, their use is strongly urged, especially on those who consider what are named tonic remedies, such as bark, iron, and the like, capable of communicating direct vigour by some special properties inherent in themselves; but to those again who recognise the truth of an axiom, perhaps the most valuable in medicine, that the true value of a remedy depends on the circumstances under which it is administered, there will generally appear reasons in this disease why these should be given with much caution.

That the impaired action of the digestive organs depends, primarily, on some obscure changes in their vital relations, producing what may be termed debility in them, is at least probable; and that the general innutrition, or weakness which pervades the whole system, and frequently in turn establishes the local symptoms of struma in the lungs or elsewhere, arises directly out of this, is as certain as any proposition connected with vitality, but it by no means follows that the medicines, generalized under the name of tonics, are those best adapted to such states. On the contrary the plain deduction from these premises appears to be, that the truly tonic plan of treatment depends on the adoption of means best calculated to rectify the original error, not by vain endeavours to obviate its effects; and hence chiefly depends on allaying irritation of the abdominal viscera, improving their secretions, and thus restoring them to that healthy habitude of action without which nutrition, and with it the vigour of the entire system, necessarily languish.

I feel called on therefore to express a strong conviction, that so long as the digestive tube remains in a congested irritable state, and so long as the abdominal secretions are improperly performed, the entire class of tonic and stimulating remedies always do harm, and that, to an extent proportioned to their strength. The case however is different when the abdominal symptoms have declined in whole or part. When the secretions have assumed an improved character, the tumidity of the abdomen subsided, and the tongue put on a more healthy aspect, then, and not till then, does it appear to me that a recourse to tonic remedies is at all admissible.

Even under such circumstances it is prudent to employ them cautiously. Infusions of the milder bitters should first be tried; if these produce no febrile excitement, they may either be used with more freedom, or an advance made to the various preparations of cinchona: iron seems to be only admissible when all symptoms of abdominal irritation have subsided; given under other circumstances, it usually aggravates the condition it is meant to cure.

Such then appear to me the leading principles which should guide our practice in the treatment of strumous dispepsia, a form of disease which indeed may exist in full intensity without being succeeded by tubercular deposition, but which, when it occurs in constitutions otherwise prone to take on that action, forms a most important element in its production. In all such cases therefore it is necessary to be alive to the probability of such event, and while we employ our best endeavours to restore the primary organs of nutrition to a healthy state, endeavour at the same time to avert those consecutive evils, towards the lungs especially, which may always be suspected, and which even early in the disease are often unequivocally announced by the signs of incipient irritation in those organs.

Now it appears tolerably certain that the mitigation, or even removal, of the abdominal affection will by no means always arrest the subsequent action of which tubercle is the immediate result, but that even during the time necessary to accomplish this, a slow progressive process is going on, out of which the pulmonary affection springs. We here therefore arrive at a point which seems to indicate that something more is wanting than a mere removal of abdominal disease, and it was from viewing the question under this aspect that I was originally led to adopt a line of practice with which, after an extensive trial of nearly ten years, I have every day more and more reason to be satisfied.

The pathological observations before delivered* lead to the conclusion that, as a result of the abdominal affection just described, the blood becomes charged with particles derived from the materials of nutrition, which being carried forward to the lungs, are capable in some organizations of passing through their extreme vessels, and hence producing no effect, but which in other cases are retained by the capillaries, and thus by gradual accumulation, form masses apparently homogeneous, to which we apply conventionally the name of tubercle.

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* Vide p. 162, et seq.

The nature of the matter of which these particles consists, seems to approach most nearly to the proximate principle albumen, and their existence is not merely theoretically assumed, but demonstrable under the microscope, in the blood, not of phthisical persons only, but often also of those labouring under cachectic states, otherwise produced. On this view tubercles in their final deposition depend on a mechanical cause, and as we cannot rationally hope to alter the imperfect capillary organization which determines this, it follows that the leading principles of successful treatment, must either depend on preventing the occupation of the blood by these abnormal particles, or, by effecting their disintegration or solution after they are formed. Of the means best calculated to fulfil the first of these intentions, I have already treated; those available towards the accomplishment of the second, yet remain to be noticed, and these are of even greater importance than the others, inasmuch as the physician is seldom consulted till the abdominal affection has for some time re-acted on the blood, and this while the appreciable symptoms are so slight as to have excited little either of attention or alarm.

In reflecting on the remedies likely to effect an object which thus appeared of great importance, I was led to examine into the degrees of solvent power exerted towards the matter of tubercle by various substances; and, as already stated at an earlier period of this treatise,* after trying a large number of solutions, none were found which, in this respect, at all equalled the alkalis in a state of purity. Independent of this, many circumstances seemed to point them out as remedies which deserved a more persevering trial in the treatment of consumption than they had hitherto received. Their universally acknowledged efficacy in the analogous affection of external scrofula, and the general impression which prevailed in the humoral schools of pathology, that they constituted the most effectual of those substances which were comprised in the class of attenuating medicines, all seemed to encourage a hope that they might also prove of advantage in subduing an affection which appeared partly to result from an abnormal condition of the blood: nor was their use in this disease by any means new, for there is scarce a writer on the subject, who has not incidentally alluded to them among the numerous medicines recommended; though I am not aware that any one has urged their employment with the same intentions, or in the persevering manner, which appears to me essential to success. Besides this, they have generally been employed when in union with carbonic acid, a state which, if the views inculcated be correct, renders them to a large extent inert, for these salts exert no chemical action at all on tubercular matter, while the pure

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Vide p. 124.

alkali on the contrary effects its solution with great facility.

Such were the theoretical views, which induced me many years ago to adopt the caustic alkalis as essential means in the treatment of consumption, but I should not have considered myself justified in directing public attention to the practice, had not a very ample experience, extending through a period of nearly ten years, and embracing above 400 cases of well marked phthisis, confirmed the soundness of the views which originally led to its adoption. In carrying this practice into execution it is important that the intentions sought to be attained should he held steadily in view; for when the nature of these is considered, it is obvious that *perseverance* forms one of the great elements of success. If, either from want of confidence on the part of the patient, or of tenacity of purpose on that of the physician, we are deterred from following them out, I cannot admit that a fair trial has been made, of means which can only be expected to exert their influence by slow degrees.

Where all the circumstances, functional, as well as organic, which co-operate in the production of phthisis, are present, it is probable that from the very first derangement of the digestive organs, there exists in the blood a portion, greater or less, of the matter which finally settles in the lungs; and it is quite certain that, in nearly every example, this last act at first takes place only to a limited extent—an extent so limited as to interfere but little with their functions; and there is great reason to believe that if the deposition was arrested at this period, those few and scattered tubercles which are then present would either remain latent, or that the powers of the system would be sufficient to extrude them successfully in a softened state. It is hence the accumulative tendency displayed by these bodies which we have the greatest reason to dread, and it is this which an alkaline treatment appears to counteract more successfully than any other. I am not indeed prepared to say that it effects the absorption of existing tubercles, though by a few of the cases afterwards given some evidence of this appears to be afforded, but I believe it often capable of checking the increase of these bodies, and thus effecting the only purpose we can rationally propose to ourselves in the early treatment of the complaint.

When therefore those symptoms are alone present, from which we infer the existence of a phthisical tendency, without any proofs of its actual presence, the objects of this practice may be termed anticipative, or calculated to avert the occurrence of impending deposition; when again evidence exists of this having partially occurred, its aim is to prevent an increase, in the hope, not unfrequently realized, that the restorative powers of the system, sustained and directed by proper management, may finally prove sufficient either to remove, or retain in a quiescent state, those tubercles which are already present.

Having named the remedy on which I am disposed to found my chief reliance in the treatment of consumption in its formative stage, it remains to indicate with somewhat more precision, the precautions necessary to be observed in its employment.

The leading points to be here insisted on, are, first, that the alkalis should be employed in their pure state, for even carbonic acid, which of all acids least destroys their caustic properties, appears to render them for our present purpose quite inert; and the second point is, that they should not be united in prescription with other medicines which from containing a free acid might change them into salts; it is in fact a *pure uncombined alkali* which is here recommended, not any of the combinations into which it so readily enters either before or during the act of administration. It seems to me probable, that either of the fixed alkalis are fitted to effect the proposed intention; but I have generally had recourse to potash, because it is more readily procured in every shop, under the form of the liquor potassæ of the Pharmacopœia. Of Brandish's* solution I have had

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* The result obtained by the formula of Mr. Brandish differs from that of the Pharmacopœia. The object of each is to afford a solution of pure potash, and the decomposition of the carbonate is effected in each case by lime.

The Pharmacopœia recommends fifteen ounces of the carbonate of potash to be dissolved, under certain precautions, in one gallon of water, adding afterwards eight ounces of lime recently calcined, and then drawing off the pure liquid.

Reducing Mr. Brandish's formula to the same quantities as little experience, but its composition differs only slightly from the other, and there can be no doubt its powers are equally great. Whichever solution be used, it is indispensable that it be steadily and long continued. In the great majority of cases, when first seen by the physician, the enemy has been for some time at work, and tubercles already occupy portions of the lungs; the symptoms therefore result from two causes; first, from the continued action of the one which produced these bodies and is still adding to their number: and secondly, from the pulmonic reaction dependant on their presence. It is a suspension of the first of these actions that alkalis appear to me capable of effecting in a slow

those of the London College, he directs one pound of American ashes, and about six ounces of the same recently procured from the combustion of ash timber, to be mixed with one gallon of water at a boiling heat, and six ounces of lime to be then added in detail: the liquor is to be drawn off slowly by a wooden spigot and faucet. He who investigates the above formula with a proper regard to the chemical changes produced, will be conducted to the conclusion that, on principle, there is little difference between them, and that if one solution be proper as a therapeutical agent, the other must be also so, in an equal degree.

And here I would say a few words in reference to Mr. Brandish, on a point which perhaps more than any other ought to be a cause of anxiety to every writer, the charge of adopting an idea without due acknowledgment. Mr. Brandish's pamphlet was published in 1811; from some cause it enjoyed only an ephemeral reputation; and I may with perfect truth profess that I had long adopted a parallel treatment in consumption of the lungs to that which it was his intention to recommend in reference to struma as it affects other organs, before I was at all aware that any such principles of treatment had before been urged.—Vide *Observations on the Use of Caustic Alkali, in Scrofula, &c. By Jonathan Brandish. London,* 1811.

progressive manner; and we hence must not be deterred from their persistent use, because symptoms continue to be present which depend on pulmonic conditions over which, when once formed, these medicines exert little influence. I offer this remark from feeling convinced that I have frequently, in my earlier employment of the practice, deprived it of all chance of ultimate success, by suspending the use of the remedy in despair after too short a trial, and from having finally succeeded in other cases, where there appeared little ground for hope, by a steady and determined perseverance.

In many instances the commencing symptoms of pulmonary disease decline with great rapidity under the use of the alkali; such I believe to be those in which the act of deposition is either just commencing, or has only been effected to a small extent; here however it is always prudent to continue the remedy for some time even after the symptoms are removed, and have again recourse to it should they recommence.

When again deposition is further advanced before our attention is directed to the case, much greater perseverance is at all times required; and from this we must not be deterred by the persistence of cough and other signs of pulmonic irritation, for these must be expected to continue for some time, even when our measures are successful in preventing an increase of the conditions they depend on.

The quantity of the alkaline solution proper to be given must be regulated by the age of the patient—the severity of the symptoms—and, above all, by the amount of consumptive tendency which he shows ; very large doses are however seldom requisite, the result depending more on sustaining its action for some time—than on an impression speedily made. With this view, children under the age of twelve or fourteen may take from fifteen to twenty-five drops of the liquor potassæ, three or four times in the day, according to circumstances, a quantity which with due perseverance is usually sufficient. In adults from a half to one drachm by measure, repeated at the same intervals, offers them whatever advantages the remedy is capable of producing.

In all cases the alkali must be employed as the fundamental part of the treatment, and whatever other means may be deemed proper to meet symptoms, or for any other reason, must not be such as interfere with its caustic qualities. On these points I shall have an opportunity of dwelling more at large in the succeeding chapter.

In a very early stage of the disease, where our intention is simply to conjoin the continued use of the alkali with the general treatment directed to the organs of digestion, we can employ no better vehicle than milk, or distilled water, in quantity sufficient to produce a due degree of dilution : by adhering to them, we obviate the possibility of the remedy entering into any combination, at least before it is received into the stomach.

As this organ perhaps at all times contains a certain quantity of free acid, some portion of the alkali must probably always be combined before it can be received into the blood; and hence where there exists evidence than an unusual acidity of stomach is present, we gain some advantage by providing, as much as may be, against such impediment to the execution of our object. This intention might doubtless be fulfilled by augmenting considerably the dose of the alkaline solution itself, but I have found it more expedient to employ its carbonate, from ten to twenty grains of this should therefore, in such examples, be added to each dose of the solution; practically I have often found this highly useful—theoretically it is not altogether absurd to suppose that the carbonate from neutralizing existing acid paves the way to the admission of its associated remedy. When an aperient is required, under such circumstances, a moderate dose of good magnesia offers the best resource.

I have thus endeavoured to point out the nature of, and the precautions necessary to be observed in, the administration of a remedy which, I conceive to be of more use than any other in the treatment of tubercular consumption. Whatever opinion may be formed of the pathological views delivered in the earlier pages of this book, is of trivial importance when compared with the general adoption of a plan, on empirical principles alone, of the extreme value of which a very extended experience has convinced me. That it, or any means, will ever prove effective in cases of consumption considerably advanced, it would be absurd to suppose; the re-action of the lungs is then so extensive and severe, as to preclude hope under any management whatever; but with all our improved methods of diagnosis I conceive it often impossible to determine where the disease has so far advanced as to render recovery an impossible event, and I consequently conclude that it is our duty in all cases to afford the only chance which seems to remain. In the large majority of cases under such circumstances we shall unquestionably fail, in a few we may reasonably expect to succeed, a fact as I conceive fully proved by certain of the cases which conclude this work.

CHAP. V.

Treatment of Phthisis in its Stage of Reaction.

PRELIMINARY REMARKS—DIVISION OF THE SUBJECT INTO FOUR SECTIONS. SECTION 1. RESTRICTION OF SYSTEMIC ACTION CONSIDERED UNDER THE THREE HEADS OF DIET—AIR AND EXERCISE—MEDICINAL AGENTS. SECTION 2. THEORY OF, AND MEANS OF ENCOURAGING, VICARIOUS RESPIRATION. SECTION 3. REMEDIES DIRECTLY APPLIED TO THE LUNGS BY INHALATION. SECTION 4. TREATMENT OF COMPLICATIONS AND SYMPTOMS—INFLAMMATORY COMPLICATIONS—HECTIC FEVER—HÆMOPTYSIS—PERSPIRATIONS—COUGH.

In the last chapter I endeavoured to trace the different links in that chain of morbid action which finally ends in the establishment of pulmonary tubercles, and to point out the practice which has appeared most influential in preventing their formation. We now arrive at a point where, assuming their actual presence, the disease is fully formed, and at which therefore attention requires to be directed, not simply to the question of formation, but to the evils which result towards the whole body as a consequence of the pulmonic condition.

In entering on this portion of our subject, the essentially progressive character of the affection must ever be held in view, for while such portions of the lung as are already involved, exert their reflected influence towards the whole frame, the action which originally produced the tubercles which they contain is, unless counteracted, still proceeding, involving larger and larger portions in the same deposit, and fast hurrying on the disease to a point, at which all attempts at cure become utterly inefficacious. This in fully formed phthisis is of all practical considerations the most important, calling on us to carry out in a persevering manner that line of treatment already noticed, which is not less necessary when tubercles actually exist, as it is where from the general aspect of the case we have only had reason to apprehend their approach. Whatever other means therefore it may be judged expedient to adopt with a view of enabling the body to bear up against impressions emanating from the local affection, they ought always to be conjoined with a continued application of the crude alkali. On the details of this practice I have sufficiently commented before; to the additional measures requisite for the cure of phthisis in its second or reactive stage it is now necessary to advert.

When we consider the various and discordant remedies recommended in consumption, and reflect how many of these have been suggested by men of character, judgment, and experience, there can remain little question that each in turn has been actuated by the best intentions; and that the entire absence of all settled principles of treatment, which we still have so much reason to deplore, depends on causes very different from want of honesty in reporting, or of industry in eliciting what each author in turn has conceived to be the truth. "We may," says Laennec, "be well assured that a disease is irremediable, when we find employed in its treatment, almost every known medicament, however different or even opposite its effect; when we see new remedies proposed every day, and old ones revived, after having lain long in merited oblivion; when in fact we find no plan constant, but that of giving paliatives, and no means persevered in but such as are proper for fulfilling indications purely symptomatic. On these grounds have been alternately cried up—alkalis and acids; spare diet, and rich animal diet; dry air, and moist air; pure air and air impregnated with fœtid vapours; oxygen, hydrogen, and carbonic acid; exercise and quiet; emollients and tonics; heat and cold; paregoric and other anodynes and stimulants."*

Of the facts stated in this quotation no one can for a moment entertain a doubt, but it does not appear to me equally certain, that the conclusion arrived at is correct—it does not appear to follow that a disease is absolutely "irremediable" by art, simply because the treatment pursued is rarely attended with success; neither does it follow, that a disease so complex as consumption, may not admit in its various stages and complications, the beneficial application of many remedies, apparently discordant in their action on the system. On the contrary it appears to me quite reasonable that this should be the case, and it hence becomes a very curious and important question to consider whether these difficulties can in any way be met—whether our present acquaintance with the laws of the animal economy justifies an attempt to reconcile conflicting opinions, and establish on rational principles the general treatment of this formidable malady. I am not without a hope that this is possible, and at all events such endeavour will afford the basis of a methodical arrangement, and enable me to present in an intelligible form such remarks on remedies and other methods of cure as it may appear desirable to make.

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* Laennec, translated by Forbes, p. 360.

In the remarks with which this volume commences, I have endeavoured to show that, viewing a living animal in its physiological relations, there is necessarily preserved an exact balance between the actions of the lung on the one hand, and of the organs on the other, the former in a stated time converting just as much venous blood into arterial, as the latter convert of arterial into venous ; unless this balance be duly preserved disease inevitably ensues.

I have further remarked that where from any cause a certain amount of lung is withdrawn from respiration, there arises an increased difficulty in forming the proper excitant of action, arterial blood; but that, assuming the function which creates it to be properly executed on the still effective portions of the organ, the resulting constitutional evils show themselves more in the limitation of action, capable of execution in a stated time, than by any enduring influence exerted towards either the functions of life, or organs of the body.

Finally, I have endeavoured to show by examining the state of the pulmonic vessels in cases of established phthisis, that where tubercles are merely disseminated or imbedded in lung, respirable up to their exterior boundary, they exert an influence, simply analogous to that which results from any other cause which equally diminishes the function of the lungs, and thus induce effects not differing essentially from those described in the last paragraph. This occurs in those cases which are usually set forth as examples of latent phthisis,—cases in which a tubercular constitution and evidence of actual deposition are present without their presenting symptoms of extreme danger or even of great importance. But in following up the same line of inquiry I have still further attempted to demonstrate, that in many examples of consumption we find tubercles not merely withdrawing certain portions of the lung from the execution of its proper function, but inducing an imperfect execution of this, in the part occupied, and as a result of this I have conceived myself entitled to infer that there must exist in the arteries of the system a current of blood only imperfectly arterialized and consequently unfitted for sustaining action or ministering to nutrition in an efficient manner.

It is to one or both of these causes, acting first on the blood and subsequently through the blood on the frame, that we must attribute the morbid phenomena manifested by all parts as a result of pulmonary tubercle, and although the leading object of practice must ever be, either to prevent an increase of the local affection, or to facilitate the removal of the foreign matter already present, yet in accomplishing this end it is of the utmost importance to counteract, as much as may be, those constitutional symptoms, which, springing directly out of the local condition, tend in turn by a reflected action to aggravate those functional errors of digestion which originally produced it.

It appears then, that even under the most favourable circumstances, there exists in consumption an increased difficulty in forming arterial blood, and therefore an increased difficulty in sustaining action, and the organ is hence called on to perform, *ceteris paribus*, an additional amount of duty at the very time, and under the very circumstances in which, as an organ, quiescence is desirable. Now it is unnecessary to say that entire quiescence of the lung is incompatible with the continuance of life, and there seems to be little question that in all its diseases, this forms one of the reasons why they are always so important, and cured with so much difficulty; but it is equally true that within certain limits we do possess the power of either diminishing or augmenting the functional activity of the organ, this being accomplished either by increasing or controling the general actions of the body.

This consideration then leads us to infer, the importance of so regulating those, as may best secure the minimum amount of pulmonic exertion, which is compatible with a due regard to the fulfilment of other indications, and leads to an adoption of the principle, of aiming as far as can safely be effected, at retaining the lung in a state not indeed of absolute, but of relative quiescence.

On these grounds I am disposed to view *the restriction of systemic action by all means which can safely be applied,* as forming one very leading indication in the treatment of phthisis in its reactive stage, and to consider many of the measures, generally acknowledged to be useful, as acting on the principle here set forth.

The special means by which the indication is best accomplished may be practically considered under three heads :

1. The diet proper for the consumptive, and the principles which ought to guide us in its selection.
2. Their management as regards air, exercise, and the regulation of the mind.
3. Their treatment as regards medicinal agents, including the theory of their action.

But independent of the principle involved in this section, there exists another question of great importance which appears to demand serious consideration.

I have inferred that at least in many cases of consumption, and those of the worst kind, the *quality* of blood flowing in the arteries is impaired, and impaired as the result of a physical condition of the lung, which, so long as it remains, renders the organ inadequate to its improvement by any acceleration of its action. Here then a difficulty occurs which the breathing apparatus, under any management, cannot overcome ; and it hence is a matter of much importance to consider, whether in such circumstances there exists any power inherent in the body, which, by the establishment of a new and substitutive action, or by the increase of one always present, may effect on the blood while flowing over other surfaces and through capillaries connected with the left heart, changes analogous to those which it ought during a state of health to undergo in the lungs only. This question will demand consideration in a separate section.

The strictly constitutional treatment of phthisis on its advanced stage, may be embraced under these two sections : its local treatment being in them left unnoticed. This, from the nature, site, and function of the organ, necessarily resolves itself into a consideration of the benefits resulting from the inhalation of various substances, a means of cure lately revived and highly extolled—certainly not without its advantages, but advantages which, in my belief, are always of subordinate importance, and can never be attained without a due adaptation of the particular vapour, or air, to the existing condition of each respective case. I shall endeavour, in a separate section, to indicate briefly the true principles which should guide us in this mode of cure; and point out, as far as experience admits, the particular substances which appear best adapted to fulfil the ever-varying indications which arise in this complicated malady as connected with its local site.

Finally, there are many points in the treatment of consumption which cannot be properly arranged under any of these heads; symptoms complicating the general affection, certainly of great importance, but which must be met as they arise, and not unfrequently met, for the time, by means apparently inconsistent with the principles of cure it has been my desire to inculcate. Of this description are:

1. Inflammatory complications.
2. Hectic fever.
3. Hæmoptysis.
4. Diarrhœa.
5. Colliquative Perspirations.
6. Cough.

SECTION I.

Restriction of Systemic Action, considered under the three heads of Diet, Exercise, and Medicines.

1. DIET.—There is no point in the management of consumption regarding which more contradictory opinions have prevailed than that of diet; while some have advocated the use of highly animalized food, and even the employment of diffusible stimuli; others, proceeding on principles entirely opposite, have supposed that a regimen strictly antiphlogistic in all its details afforded the only chance of success. The former opinion is advocated by those who have considered phthisis in its origin and end a disease of pure debility: the latter by those who attribute all its evils to an inflammatory process.

I have already offered my reasons for believing that neither of these opinions are correct, when exclusively maintained; that the debility is to be considered as an effect, not a cause, of the affection; while the symptoms of inflammation, without question often present, must be viewed as the manifestations of an associated condition arising out of and engrafted on the main disease.

Holding these points in recollection, and coupling them with what has been already said, of the advantages which result from a proper regulation of action where the lungs are imperfect, it may not perhaps be impossible to arrive at some settled principles for our guidance, on the question of diet, between the two extremes above noticed.

There is no fact in physiology better established than that the act of digestion augments for the time the excitement of the heart and lungs; this is an unvarying law, liable to no exception—neither is any one more certain than that the amount of this excitement, even in the most healthy body, bears reference to the quality of the food employed; that which is most rich and animalized being also more stimulating, causing the animal to consume more oxygen, or what amounts to the same thing, imposing an increase of duty on its respiratory organs.*

It is again certain that where from any cause the digestive and sanguifying functions are impaired, the excitement produced by the act of elaborating food is greater than where these are healthily performed, so that the same diet which in the one case only slightly accelerates pulmonic action, does so to a much greater degree in the other.

This probably depends on two causes:—first, on the increased difficulty with which aliment is converted into blood, and secondly on the irritation caused by portions not properly resolved in the stomach, in consequence of its weakened condition as an organ.

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* "Diseased lungs are exasperated by a certain diet, and pacified by one of an opposite kind. The celebrated diver, Mr. Spalding, observed, that whenever he used a diet of animal food, or drank spirituous liquors, he consumed in a much shorter period the oxygen of the atmosperic air in his diving bell; and he therefore had learnt from experience to confine himself upon such occasions to vegetable diet. He also found the same effect to arise from the use of fermented liquors; and he accordingly restricted himself to the potation of simple water. The truth of these results is confirmed by the habits of the Indian Pearl-divers, who always abstain from every alimentary stimulus previous to their descent into the ocean. Those physicians who have witnessed the ravages of pulmonary disease will readily concur in the justness of these views."—*Vide Treatise on Diet, by J.A. Paris, M.D. p.* 70.

We are hence so far conducted to two principles which ought to regulate diet in diseases of the chest. First the propriety of employing those articles which are identified with the body at the least expense of action by the lung, and secondly of using those which are converted into healthy chyle with greatest certainty by the stomach.

Did the question of nutrition in consumption therefore resolve itself simply into those articles of food, which are susceptible of final deposition in the organs with the least possible exertion of the lung, we should obviously be led to conclude that those persons are right who inculcate the adoption of an antiphlogistic regimen in all its strictness. But this assumes a different aspect when we consider the actual position of patients labouring under phthisis in its advanced stage. It is indeed true that we are called on to protect their respiratory organs from an excess of action, by all legitimate means at our command, but it is also true that we have to support and renovate if possible a decaying organization, and that in accomplishing this purpose we are compelled to present to the digestive organs, articles containing as large an amount of nutritious elements as may be compatible with a due regard to the leading principle involved. These views, theoretically concluded from the physiology of digestion, are in my conviction amply confirmed by the results of experience, and most observers will be found to agree, that a diet moderately nutritious and sustaining, supplied at such intervals and in such quantities as the existing state of the stomach will admit, forms the best general rule which we can adopt in the stage of the disease now discussed; by adopting such a plan we communicate to the wasted frame the utmost amount of nutriment which its weakened organic functions are able to elaborate; while we at the same time effect this with as small an expenditure of vital power, and consequently as small an augmentation of pulmonic action, as the circumstances of the case admit. We possess no form of diet so well calculated to effect these intentions as one of which milk constitutes a considerable part, affording as this fluid does, a large amount of bland nutriment readily assimilated, and that, with but slight stimulation of the breathing apparatus. Nor is this by any means a theoretical conclusion; whether we appeal to the records of medicine, or to the popular conviction which has prevailed in all ages, and in all lands, we find its advantages authenticated by the strongest proofs, and among all the medical theories, and medical appliances which have so lavishly prevailed, it, nearly alone, has stood the test of general experience. It is not however very uncommon that with all these claims to attention, the physician finds himself baffled in carrying out his intentions, from a feeling on the part of patients that a milk diet does not suit their stomach. That this may be sometimes the case it would be unreasonable to question, but I believe it to be more rare than is generally supposed. The stomach indeed, like all organs, acquires certain habitudes of action which it is difficult at once to change, and these in consumptive cases are generally formed, long before an apparent necessity arises for a systematic regulation of the diet. Hence it often happens that when such contingency occurs, the invalid is not always disposed to exert even a small amount of self restraint, and to consider, that a change of food not precisely agreeable to his taste, is in reality obnoxious to his digestive organs. In combating such feelings an appeal to reason can alone be successful, and if this prevail, I am disposed to think that there are few constitutions in which the resolution required will not in this case receive its full reward.

Several varieties of milk, as those of the mare, goat, ass, sheep, and even that of the human female, have been recommended by different authors in certain stated quantities;—more in the form of medicine than nutriment, as if each possessed some specific virtues of its own.

This I conceive to be an entire mistake, and that the resulting benefit depends merely, on the introduction of nourishment in such a form as enables the assimilative powers to renovate the organs without difficulty or excitement; it therefore follows that, to lay down decided laws as to quantity, or form of administration, would be affecting a precision which it is impossible to attain; in these respects each individual case must stand on its own foundation.

But milk cannot constitute the sole diet of the consumptive patient, neither would it be proper that he should be excluded from articles of a more solid kind. It is here that the whole class of farinacious aliments apply so well, offering not only the means of varying the form, almost infinitely, under which milk may be agreeably taken, but in themselves ministering to nutrition, with an efficacy only second to the other. To discuss in detail the various substances comprised under this head appears unnecessary, and to attribute peculiar virtues to one above another, unwise; provided the principle be maintained, the selection ought to restwith the patient. Few physicians would be disposed to insist on the use of sago, instead of arrowroot, or urge vermicelli in preference to tapioca—or a dozen others, when his patient's palate led him to adopt a contrary conclusion.

As analogous to a farinacious diet, may be briefly named, the vegetable mucilages, or rather jellies, derived from different varieties of lichen, long prescribed as medical appliances, but which really appear to owe whatever virtues they possess, to the non-stimulating character of the nutriment they afford. Of these the L. Islandicus first introduced by the Danish Physician, Schoenheyder, late in the last century, is the one best known, and most extensively employed; other varieties, indigenous to our own country—especially the L. Pulmonarius, and L. Cocciferus, have been also suggested, but obtained less of popular favour, probably on account of their greater bitterness. The entire of those, it is well known, produce on boiling a tenacious jelly, which being given under the name of medicine, may sometimes induce patients to imbibe a nutrient article, well adapted to their condition, which applied under the name of diet, would not have been so readily received. Experience appears to show, that beyond this they are not possessed of any peculiar virtues. Of the garden snails, once so strongly recommended by Pascal and other Continental physicians, and by our own countryman Dr. Moses Griffiths, it is almost unnecessary to speak; the scepticism of modern times would probably repudiate the nauseous prescription, and it is at least consolitary to reflect, that analogous materials, more elegant, and equally efficacious, are at the disposal of the invalid.

Milk, well baked wheaten bread, and farinacious aliments, in some one or other of the various forms which the art of cookery so liberally supplies, should thus constitute a large portion of the diet in all cases of phthisis, and the entire of it in those, where symptoms of associated inflammatory action prevail. When however this is not the case—where the acceleration of the pulse and respiration obviously depend on impeded action of the lungs alone—where the emaciation and debility are great—and where gastric irritation is not present to any marked extent—in such examples there seems a call for a nearer approach to that food, which in health is the most sustaining and nutritious.

The first step in this advance, which experience appears to sanction, is to fish, and eggs, taken moderately, once a day, or once in two days, as the circumstances of the case permit. Of white fish I am myself disposed to form a high estimate. It combines many of the nutrient qualities of animal food with a greater facility of digestion, and less subsequent excitement, and altogether appears to offer a medium diet, well calculated to meet our wishes. The forms under which it is employed ought to be either that of soup, or plainly boiled, and eaten with only a small allowance of either condiments or butter. Fried fish is objectionable on account of the grease in which it necessarily is enveloped.

When the assimilative powers without any great increase of excitement appear capable of applying to useful purposes the augmented elements of nutrition with which they are furnished, there are many cases in which a more full animal diet may be advantageously permitted; poultry, game, and similar articles, are those beyond which prudence generally dictates that we should not advance. The solid fibre of beef, mutton, veal, and especially of pork, are rarely if ever proper, but soups made from the three first, when entirely deprived of fat, and largely combined with farinacious articles, often effect a useful purpose. It is however impossible to lay down any general rule on such a subject, the nearest approach must be founded on a reiteration of the principle maintained throughout this section, *that the use of a diet as highly nutritious as can be assimilated without leading to any exaggerated action of the breathing function, either directly or indirectly, is the one at all times most proper*; the moment this is exceeded we may feel well assured, that however sustaining the food employed may in itself be, aggravations will follow its adoption.

Of the class of diffusible stimuli, the opinion of the author will be inferred from the tenor of his preceding remarks. If it be improper to employ a diet capable of augmenting action in the respiratory organs, it must be still more so to prescribe those fluids, which leading to this in a much greater degree, do not even possess the advantage, except in some cases, of supplying nourishment to any extent whatever. Wines and ardent spirits may be especially arranged under this category, and it is the more necessary to offer a caution against their employment, because the debility and exhaustion generally present, offer considerable inducement to their use. The best point of view in which this question can be placed, involves a very simple idea; that wines and ardent spirits contain absolutely no *nourishment*—that they merely stimulate, but do not sustain—and that the temporary and fallacious vigour they confer is always connected with increased action of the heart and respiratory organs. It requires no depth of knowledge to comprehend this truth; the obvious acceleration of the pulse and breathing consequent on their use is familiar to all: let us apply this to diseased conditions of the lung, and the deduction must be obvious.

The same argument does not so strictly apply to malt liquors, as to wine and spirits; in one respect these are to be viewed as infusions of grain, containing a considerable share of vegetable nutriment well adapted to the wants of the consumptive, but associated with an amount of alcoholic stimulus, proportionate to their strength and richness. So far, in strict accordance with our principle, their use might appear objectionable, but experience shows that their advantages often counterbalance the evils they might be presumed to involve. Where there is no tendency to pulmonary hœmorrhage—where the pulse is weak and irritable, and only numerically quick—where fixed pain in the chest is absent—and no inflammatory symptoms prevail—in short, in examples of purely chronic phthisis, I have always found, that a moderate quantity of good ale may be employed with advantage. The quantity should not exceed from one half, to one pint in the day, the larger portion being taken at dinner, and it should always be "mild" as the term goes, or in other words contain as small a quantity of free acid as possible. In the practice of Mr. Brandish, it appears to have been the favourite vehicle in which he administered the alkaline solution whose advantages I so strongly advocate. This I conceive to be objectionable on two grounds; first, we never can be certain that the medicine is not neutralized, because in general practice we cannot command a due attention to the character of the ale employed; and secondly, because it is always unwise to associate medicine and diet. There is sufficient suffering in disease itself, without denying to the patient, even during meals, an abstraction, from the not always agreeable ministrations which his case may imperatively demand.

It is not improbable that the standard of diet which I have advocated in the preceding pages may to some appear too low, while a few will perhaps be inclined to maintain an opposite opinion; the former belief I have chiefly found among physicians, whose position, like my own, leads them to extensive practice among the poor and ill fed. This class of patients, as they appear before the medical man for prescription only, being otherwise dependant on their own resources, are little likely to err on the side of over feeding, and the great difficulty to be encountered in their treatment often depends, on a want of the nutritious diet which their situation demands; but the case is different with the same class when admitted into public asylums, and it is there that, if I mistake not, erroneous conclusions are sometimes arrived at in regard to the benefits of a high and stimulating regimen.

In such establishments it is no uncommon thing to find patients apparently improve under a diet of animal food, porter, ale, or even stronger stimulants, combined with a medical treatment tonic in all its details, and it is hence concluded that such treatment is *per se* best adapted to the advanced stage of consumption.

Such opinion does not, however, appear to be strictly warranted by the observations on which it rests. There is a vast difference between the circumstances of a phthisical patient who presents throughout to the physician a choice of all those medical means and appliances which may be deemed requisite, and such cases as those to which I have just alluded. The poor consumptive patient, having struggled long against his fearful malady, ill clothed, ill lodged, with scarce the common necessaries of life for his daily aliment, becomes as a last resource the inmate of an hospital; the first effect of this is to place him in an improved condition; he is better habited, cleanly bedded, and breathes a more equable—probably a purer atmosphere, than he did at home,—all changes, which of themselves create a temporary re-action in his favour. He now embarks on a diet highly nutritious and stimulating, applied to a constitution previously exhausted from the absence of proper aliment, and the rally which follows, too frequently a temporary and deceitful one, is referred to the treatment employed, without a proper reference to the circumstances under which it was applied.

I allude to examples of this kind because they are very common, and because they go, in my belief, to establish confidence in a practice not less improper, than the bleedings and low diet which have often been inculcated by adverse schools. Could we penetrate to the temple of truth, analyze, and estimate at their due value, the numerous circumstances which complicate all medical conclusions, we should probably find, that the benefits resulting from the practice I have named, are temporary and fallacious, and itself in ill accordance with the true principles of treatment which reactive phthisis properly demands.

A few words alone remain in addition to those already written on the question of diet and regimen in consumption. What has been previously said, I could desire to be viewed as exclusively applicable to those cases which offer a reasonable probability of ultimate success. It unfortunately need scarce be urged, that there are a vast number brought either for the first time under the care of the physician, or which have advanced in their fatal progress, despite his efforts, to a point at which such probability no longer exists. In these, the soothing of painful sensations—the combating of symptoms to the utmost of our ability—and the gratification of such tastes as long habit may have engendered, or the caprices of a wasting malady induced, is the best and wisest practice. Here, we must consequently often adopt a regimen altogether different from that which I have held to be curatively proper; stimulants require frequently to be used with great freedom, and the whole class of narcotic medicines constitute our most valuable resource, while the selection of food should be in a great measure, if not entirely, left to the sufferer himself. Let it however be remembered, that I here allude to examples in which "Hope" herself fails longer to "spring exulting"—examples, where paliation, not cure, forms the sole object of our imperfect art.

2. EXERCISE.—While, as already shown under the last head, the diet proper for the consumptive has led to many differences of opinion; the same remark applies with even greater force to the question of exercise. The well known remark of Sydenham, that "Peruvian bark is not more certainly curative of an intermittent fever, than riding is of a consumption," long continued to sway the minds of those, who were justly influenced by the experience of that great and observant physician. Dr. Rush, following up the same view, assures us, that severe exercise is the only remedy—medicines of all kinds being entirely useless; and asserts, that the hardships inherent on a military life, have cured numerous cases of confirmed consumption. Salvadori, even goes farther. He commands his patients each morning to climb a high ascent, till clothed in perspiration, and then to encourage this by the use of external warmth. Those who have

seen the consumptive patient placed under such circumstances, and watched his distressed, laborious breathing, will not conceive the prescription very easily fulfilled.

Different opinions have been held by other authors. Cullen objects to active exertion of any kind: riding he considers bad—and even riding in a carriage improper—unless on very smooth roads, obviously recognizing the principle, that all augmented activity of the muscular power is deleterious. The same opinions have been held by many others. Amid such conflicting testimonies, it is no easy matter to decide, but it appears possible that some advance towards the establishment of a rational practice may be made, by adhering to the principle already applied to the question of diet under the preceding head of this section.

When discussing the physiology of circulation at an earlier period, I endeavoured to show, that there is no function of animal life, which so augments the velocity of the pulse and respiration, as muscular contraction. The mechanical cause assigned to this, by referring it to the increased flow of venous blood, consequent on the pressure of acting muscles, appears to be altogether an inadequate explanation; and I have therefore sought for one more in accordance with just views of physiology, on the principle that as contraction is dependent on the contact of red blood with the integrant fibre of the muscle, so the exhaustion of this fluid must necessarily accord with the amount of contraction executed. This, in truth, is not strictly an explanation, because it still leaves us ignorant of the relations between vital blood and the muscle it excites: it is simply the assumption of a final cause, in itself inexplicable, but which, being assumed, enables us to comprehend subsequent variations in the vital motions of those organs, which prepare and distribute the fluid, on which action is dependant.

Waving however all theory, and looking merely to the manifest phenomena of muscular contraction, no fact is more distinct, than this—that when the muscles are at rest, the respirations and contractions of the heart are performed with comparative quietness and regularity; and that these increase in a precise ratio with the augmentation of muscular effort, till a point is finally attained, at which the organs effecting it either act with diminished energy, or cease to act altogether—not on account of any structural imperfection—but because they no longer receive a sufficient supply of their proper excitant. Now, the facility with which arterial blood can be supplied to the muscles is great, in proportion to the extent of the ratio which, as regards volume, the lungs bear to the system, and conversely small, in proportion to the smallness of that ratio. Hence it happens that persons, of contracted chest, whose lungs are still healthy, become incommoded by slight muscular effort, and what is of more importance here—that persons whose lungs are occupied by disease of any kind, but more especially by tubercles, are absolutely incapable of exercising an amount of muscular effort, which would have little influenced the acts of circulation or respiration under different circumstances.

The fact therefore is, that every increase of muscular contraction, imposes an additional duty on the lungs; and we are hence led to conclude on this ground alone, that where it is proper to retain those organs in an unexcited state, the intention is invalidated by throwing the muscular system into powerful action. The justness of this physiological conclusion appears to be confirmed by the results of experience, and those observers appear to be correct who have recommended phthisical patients to avoid as much as possible such exercises as involve considerable bodily exertion, but who at the same time advocate the use of those, which without augmenting directly the action of the muscles, and through them indirectly of the lung, still enable the patient to enjoy such changes of scene, and air, as are essential in the management of all chronic maladies.

The modes in which exercise may be taken, it requires not to be stated, are extremely numerous, each involving a variable amount of muscular effort. Hard walking, or an active life devoted to mechanical pursuits, perhaps involves the greatest. Riding on horseback may be assumed as following next; because, though in some measure the rider may be called a passive agent, his muscles are still called into considerable activity, greater or less according to many circumstances. Riding in a carriage leads to less of muscular exertion than the last—and is altogether more in accordance with the principle advocated; yet, even here, Cullen is probably correct in stating that a "smooth road," I would add, an "easy vehicle" also, should be selected. Sailing, whose benefits have sometimes been attributed to a virtue inherent in "sea air"—sometimes to the nausea often produced—and sometimes to the passive motion it implies,—seems to be a mode of exercise which is highly proper, provided always that it is judiciously employed as an exercise, and connected with those habits of life, and that treatment which is otherwise proper; it is thus that the advantage of sea voyages is extremely questionable, connected as they necessarily are with confinement in cabins, crowded by many human beings; while sailing, or being rowed in an open boat on a lake, or the sea, is often attended with great advantage. Swinging is another mode of passive motion, extremely analogous to the last, and whose benefits are scarcely if at all inferior. *In both cases the air enveloping the patient is constantly renewed without the patient himself being an active agent in the renewal;* of this I shall have occasion to speak afterwards, but may here remark that the words printed in Italics point to a principle which is of no inconsiderable importance.

The general result of experience thus seems to be that passive motion is the form of exercise best adapted to the treatment of consumption when fully established. The physiological cause to which this practical conclusion may be referred, I have attempted to point out; but whatever view may be taken of that explanation, there are, I think, few physicians who, having watched the hurried respirations, inordinately rapid pulse, and general distress of phthisical patients, under muscular excitement, who will be disposed to retain a confidence in the efficacy of those more violent exercises which have been frequently and strongly recommended. Whatever advantages may be supposed to accrue, and which frequently do accrue from the use of means somewhat vaguely generalized under the names of " air and exercise," are far more than counter-balanced, by that high action of the muscular system too frequently employed in their acquisition. Luckily we possess the means of reconciling those difficulties, when the circumstances of the patient permit; by passive motion practised in a free and temperate atmosphere, he may be enabled to avail himself of all the advantages which it is able to confer, without imposing on the respiratory organs, that high degree of action which considerable muscular exertion unavoidably induces.

The same physiological reason which explains why it is advisable to avoid all high action of the muscles in phthisis, applies with equal force to over-excitement of the mind. Of the nature of that mysterious connexion which exists between mind and matter, we indeed know nothing, but we still have it on the very best evidence, that, precisely as the organic and vital functions of the body depend for their excitement on the constant flow of red blood, so also is this condition absolutely requisite for sustaining the action of that portion of the brain, which is obviously connected with the execution of intellectual acts; and in either case we also know, that the consumption of this fluid is directly regulated by the amount of function performed. This reasoning leads us to conclude—what indeed is fully borne out by observation—that quietude of mind, abstraction from the cares and anxieties of life, and an avoidance of all those mental efforts which excite the brain, are of high importance in the more advanced stages of phthisis. The patient should be hence amused by cheerful conversation, and abstracted as much as possible from a continued reference to his own situation, by the soothing influence of social intercourse; study of any severe or abstruse character ought to be carefully shunned, and the excitements of a professional or business life as much as possible avoided; in short, every means should be adopted which tend to retain the mind in a state of quietness and repose.

3. Medicinal Agents.—The leading distinction in the opinions of men on the dietetic and general management of consumption, has been, as we

have already seen, the unavoidable result of the different views held as regards its pathology. The same remark applies with equal force to medicinal agents, and we hence find, that while tonics and stimulants have been lauded on the one hand; depressments and sedatives, including blood-letting, the most powerful of all, have been equally extolled upon the other. It is certain that there are many cases which from peculiarity of symptoms demand a nice adaptation of both these adverse treatments, but the important question is, which of them is generally applicable to the essential nature of the disease itself; it is quite impossible that, in this respect, both can be correct, but by no means impossible that circumstances may arise during the progress of a very complex malady, where a combination of the two are necessary. These are points of deep interest, which without merely regard to individual or specific remedies, may fairly be examined on the principles of general therapeutics. It will be inferred from the tenor of the previous pages that under certain limitations, the class of sedative remedies accords best with the fulfilment of the object which I have considered it of the highest importance to attain—the restriction, namely, of systemic action, with the ultimate view of diminishing the necessity for action by the lungs. There are numerous remedies which effect this object, though it is much more difficult to say *how* they effect it, the action of medicines being perhaps the most obscure point in medicine; we simply know at present that they *do* effect it, and that hence they are available remedies, provided the principle on which they are given be in itself correct.

Of these medicines, the great majority appear to act by a primary and appreciable impression made on the stomach; while the action of a few, especially digitalis, and the hydrocyanic acid, cannot be explained in this way, but must be referred to some unknown influence exerted by them directly on living textures, whether through the medium of their nerves or vessels.

Of the action of this class of medicines we therefore know absolutely nothing; of the action of the others we know a little more. We know that a substance which produces nausea or excites vomiting, will depress the energies of the heart and lungs; but we do not understand, and cannot explain, how such a condition of stomach should lead to this result; this is a distinction which some ingenious authors, who endeavour to explain everything, appear to have forgot. To say that it depends on "sympathy," is merely to express the fact in other words, but is certainly not an explanation. To throw aside, however, all speculative discussion, there can remain no doubt of the fact, that nauseating remedies, when they have produced their effect, do lower the force and velocity of the circulation, and do decrease the action of the lungs, and in support of the principle for which I contend, it is at least a curious coincidence, that from the earliest records of medicine down to this day, the large majority of reported cures of phthisis have been connected with the use of nauseating and emetic remedies. This does not rest alone on my own authority. "It is remarkable," says Dr. Young, in the most learned compilation of opinions on the question of consumption which ever has been produced,—"It is remarkable that a very great majority of the cures of consumption which are related by different authors, have either been performed by emetics, or by decidedly nauseating remedies, although their full action may not always have been exhibited; and we may include in this description, not only ipecacuanha and the sulphates of copper and zinc, but also the digitalis, sea voyages, and swinging."*

I am not disposed to allow that the mode of action of all the remedies named in the above quotation is explicable, on the principle of nausea alone; on the contrary, sailing and the swing, are often of great advantage, where no such feeling is produced, and it occurs to me that their benefits ought more to be referred to passive motion, and the constant application of fresh portions of air to the surface of the body; of this I shall have occasion to speak in a future section.

All however will agree with Dr. Young that general experience fully attests the great value of remedies capable of producing an emetic effect; but there is a question of some importance, how far their influence is exerted through the act of vomiting, and how far on the simple production, of the sensation which precedes it; both result from the use of the same means, and by both, action is for the time diminished, and it is to this I feel disposed to refer their extensive utility in all cases, more especially in cases of acute consumption; but so far as my experience goes, this end is better attained by not pushing the medicine to the extent of evacuating the stomach. Of the numerous special remedies capable of inducing nausea, ipecacuan is perhaps the best and most manageable which we possess; the emetic tartar is objectionable, and often dangerous from its decidedly irritating effects on the mucous coat of the abdomen, while the sulphates of copper and zinc, however useful, where full and speedy vomiting is required, are but ill calculated to effect the object at present under consideration.

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*Vide Young's Treatise on Consumption, p. 64-5.

The best form in which ipecacuan can be given is, its saturated solution in proof spirit; to its solution in wine—the common one sold in the shops, I of course object, from almost invariably prescribing this and other remedies, along with the caustic alkali; and attaching, as I do, such paramount importance to this being given in a state of purity, any associated solution containing a free acid,—as vinous preparations always do, is obviously improper. Where narcotics are indicated, ipecacuan in substance may be advantageously combined, and the same may be said where slight mercurials or alteratives are demanded by the condition of the abdominal viscera. The quantity of the drug employed can scarcely be defined ; much must depend on the circumstances of each case, and especially on the tolerance of the patient's stomach; the only general rule which can be laid down is, to excite a gentle nausea, frequently. To effect this purpose from ten to twenty drops of the saturated tincture generally suffices, and this may be repeated three or four times a day. The utility of the class of remedies here discussed, has by no means always been referred to the nausea they excite and the consequent restriction which this imposes on action ; on the contrary, many have considered the act of vomiting as essential to their efficacy. It would be endless to enumerate the authors who have held this opinion, sometimes urging it empirically, sometimes on the principle I contend for here—and at others, on their evacuating the material cause of the chest disease. The practice has of late years been strongly advocated by Sir James Clark,* who following the pathology of Dr. Carswell,† is disposed to explain the theory of their action on two principles, first, that they expel tubercular matter which is presumed to be poured out on the mucous lining of the bronchi or air cells—and secondly, that they augment the bronchial secretion, and in this way co-operate in its removal. Assuming Dr. Carswell's views of the seat of tubercle to be correct; and supposing, as these do, that the foreign matter has no very permanent or strong adhesion to the living tissue, it is quite conceivable that the act of vomiting might mechanically aid in its extrusion: but believing as I do, that it is contained *within* the minute vessels which ramify on the mucous membrane, there appears no very reasonable hope that it can be got rid of in such a manner, and in so far as my experience goes, the results of practice confirm this conviction. Independent of this, though the nausea which precedes vomiting restrains action for the time, the act itself frequently, if not always, augments it afterwards, on the common principle of reaction; a circumstance which operates as a strong contra-indication to the full emetic treatment. In some cases, however, and to meet a temporary difficulty, gentle emetics are not unfrequently of use. Where a copious evolution of softened tubercle, and a large quantity of bronchial secretion, which usually accompanies it, are with great difficulty extruded by the natural efforts, the shock of vomiting is often necessary to free the tubes and cells from the obstruction, and permit the air to enter with greater facility into whatever portions of lung may still be capable of performing their function.

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* Vide "Clark on Pulmonary Consumption," pp. 348-351.

† Vide his "Illustrations of the Elementary Forms of Disease Fasciculus," No. 1.

While many medicines which restrain action thus evidently operate by producing a primary and manifest effect on the stomach, several others, as already stated, effect the same object, without our being able to offer even this explanation. The chief of these are the hydrocyanic acid, and digitalis, both of which, especially the latter, have obtained reputation in the treatment of consumption. That the former of these possesses the power of repressing action even to the extent, when largely given, of striking at the very fountain of vitality, is undoubted; and, that it ought hence to present a most valuable agent, for carrying out the intention to which this section particularly applies, would necessarily follow; and I doubt not that where it has been employed with advantage, and of this numerous cases are on record, it has been so on this principle. But believing as I do, that in consumption we have not merely to encounter the evils which depend on the re-action of tubercles already established, but that the great control which we possess over the disease depends on preventing their location, it follows, that if a remedy found equal to the fulfilment of the second purpose, is chemically altered by its union with another remedy, however well adapted to the first, that its use can only be advocated under considerable limitations. This is the position of hydrocyanic acid. That by reducing action it is often a valuable agent in *re-active* phthisis, cannot be doubted; but that as an acid, it unites with, and changes the properties of the alkali, which I have alone found of use in *formative* phthisis, is equally certain, and thus the loss being in my conception greater than the gain, I have always felt disposed to trust to other means which conduce to the same end as it, without the same disadvantages.

Digitalis in many instances lowers the pulse, and with it the respirations. I shall not enter on the abstruse and often discussed question of whether it does so by primarily acting as a sedative, or by secondarily diminishing action as the result of its previous excitation—its sedative effects are at least much more usually perceptible than its stimulant. Of its utility in phthisis very different opinions have been held; elevated at times into an unmerited reputation, by the enthusiasm of Beddoes, and often degraded below its real merits, by others, it has like all remedies been the sport of opinions, in an uncertain art. Truth, here, as in other cases, may probably be found to reside midway between the two extremes of censure and of praise. It certainly acts at times with great power, in controling the circulation, and where this effect follows, is of marked benefit in consumption. As an available remedy however, it has one great disadvantage—the extreme uncertainty of its action ; small doses at times inducing a very unlooked-for effect, while at others, large ones, produce scarcely any sensible influence ; and I have not found that we are capable of inferring from peculiarities of symptoms, or temperament, in any special case, the probable effects of its administration. This is a serious practical evil, which, could we overcome it, would without doubt render it in consumption a remedy of great importance.

It may appear singular that in treating of a disease in which so many remedies have been recommended, I should in a great measure have confined my remarks to one description only. The apology must be, that throughout these pages I have endeavoured to illustrate principles, without advocating special means, except in so far as the end was also held in view; acting under a strong conviction that our usual inefficiency of practice in consumption, depends not on the want of medicines, nearly so much, as on the absence of knowledge of the objects towards which they ought to be directed.

There is however another class of remedies, separated from the last by wide limits, to which it is proper to refer; I mean the class of tonics.

To advocate a recourse to these on the grounds of the emaciation and debility which accompany re-active phthisis, would be going against every principle of treatment which I have urged in this book; many of its pages already contain a record of the opinion, that debility is here the necessary result of the local affection, and can only be cured by its removal, and it would be at variance with such belief to suppose, that any tonic, should directly possess the power of communicating vigour under such circumstances. But though this be certainly true, there is often present an atonic condition of the abdominal organs, in which—always assuming them to be free from marked signs of congestion or irritation—these medicines are undoubtedly of use by improving the digestive function, and, when combined with a proper diet, leading to the more perfect elaboration of a healthy chyle, which nourishes without exciting.

Of the various tonic remedies, the lighter bitters appear to me the best. Gentian, Calumbo, Cascarilla, and many others, all offer valuable adjuncts to other and more effectual means; and in their infusions, present convenient vehicles for the administration of what I consider our great resource—the alkali. Care should be taken that the infusion employed has been recently prepared, as any evolution of free acid, the result of fermentation, renders it no longer a fitting vehicle for the associated remedy. From the Peruvian bark, or Quinine, I have never seen any particular benefit derived. The denunciations against it in consumption have often been too lavishly employed; those again who have extolled its virtues analogically, and considered it capable of arresting the paroxysm of hectic, because it arrests that of intermittent fever, would appear to have formed a very erroneous conception of the two diseases. Its infusion is often useful, like those of the milder bitters, but seldom more so.

On the utility of narcotics in phthisis, much difference of opinion has prevailed; the leading objection to them has been founded on a belief, that they tend to check expectoration, and thus lead to greater distress, than results from any benefits they confer, by diminishing pain, or soothing irritation. In my own experience I have not found this to be the case to any inconvenient extent, and it can at least be generally prevented, by uniting them with ipecacuan. Either in this form, or alone, they diminish action by moderating sensibility, and so co-operate to no small extent, in attaining the object to which so much importance is attributed throughout this section.

Of the various narcotics, opium, in its different preparations, is the most powerful, but not always the most manageable. The salts of morphia, and the sedative solution, known under the name of Batley's, seem to be the forms by which its sedative properties are best attained with the least risk of stimulation. Of the less powerful narcotics commonly employed, the extracts, or tinctures of conium and hyociamus, appear to be the most available, but of all, none I feel convinced answers so well in phthisis as the *pure* "extract of lettuce," made by the simple inspissation of its milky juice; the preparation however sold in the shops under this name is generally a very inert—I might almost add, useless preparation.* On stimulants in the later stages of consumption a few words may suffice; my opinion of them has already been sufficiently expressed. Where our intentions are still influenced by even a possible chance of cure, their use seems to me on every rational principle improper, unless to meet symptoms of temporary duration, and they ought then to be abandoned or modified so soon as the difficulty is for the moment overcome. Where again life is obviously drawing to a close, where its glimmering flame is burning feebly and uncertainly in the socket, it may doubtless be by them sustained a little longer; but as certainly not resuscitated by their use.

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* It is well known that the elder Duncan of Edinburgh, took great pains, many years ago, to introduce the genuine extract of lettuce as a remedy, and in fact it now occupies a place in all our Pharmacopœias. But we might here well exclaim, "What's in a name?" The extract of the shops is about as different from the genuine, as opium would probably be, if prepared by compression from the stalks and capsules of the poppy. I can speak to this experimentally, from having used the remedy, made by my own hands, and contrasted its effects with that procured by the process which the Pharmacopœia recommends. This is one

SECTION II.

Can a Function analogous to that of Respiration be partially performed on other Surfaces than those of the Lung?

I commence this section under the sanction of a query, from being fully aware that much difficulty exists in satisfactorily demonstrating the truth of the proposition it involves; and still more,

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of many cases, in which the expense of labour, and the consequent temptation to adulterate remedies, in themselves good, tend to degrade the science of medicine to a mere trade. The reputation of the physician, and the interests of his patient, are I fear not here only, sacrificed in some measure on the altar of the idol—Mammon.

For ample information on the subject of "Lactucarium," I would refer the reader to the Appendix of "Observations on Pulmonary Consumption, by Andrew Duncan, sen." Edinburgh, third edition, 1820.

should the question be affirmatively answered, of determining, how far such action of the living system—or if the expression be admissible—such supplemental respiration can be rendered available in the treatment of pulmonary disease. There are, however, so many facts which tend to prove that such a function is performed by the skin and other textures, that it does not appear irrational to suppose, that it may exert some influence on the blood, and through it, on life; and should this be admitted, it then becomes a matter of importance to investigate how far we possess the power of encouraging or increasing such an action; and also, whether the benefits of some approved remedies and modes of practice which have been long empirically employed, may not be in truth explained by a reference to the principle involved. To *prove* this, I am aware, would be a hopeless expectation, and even to hint at it, may to some appear a foolish one, but in a disease of such extreme fatality, and in a science where principles are so unsettled, every suggestion, not absolutely absurd, may be reasonably offered; and in the midst of much that is obscure, an idea may possibly be thrown out which admits of a more extended application in the hands of others: the towering oak is the produce of an acorn, and the baby grows into a man.

There appears to exist an intimate connexion between the theory of cutaneous respiration, and the action of those remedies which receive the name of refrigerants; for if indeed we are at all entitled to speculate on the mode of action in either case, we must do so on the assumption that the communication of oxygen to the system has no small share in it. It had long been known as a fact—known indeed from the very earliest periods—that certain medicines possessed the property, during excited states of the body, of moderating the action of the heart and arteries; and as this was always coincident with a diminution of animal heat, the appropriate name above alluded to, and merely expressive of the fact, came to be applied to them: previous to the discovery of oxygen, the power which they were assumed to possess, could only be explained on the supposition, that they diminished in some unknown manner the temperature of the stomach, and so acted, through it, on the heart, by sympathy, a word which forms, and always has formed, so convenient, though unfortunately, so indefinite a mode of getting rid of every physiological difficulty. But when oxygen was discovered, and when the immediate connexion between its consumption in the lungs, and the heat of the body, came to be established, then, men began to apply the fact which chemistry had revealed, to explain in a probable manner, and on a consistent theory, that which observation had already indicated, namely, that certain substances when received into the stomach, reduce the temperature of the body, and along with this, diminish the force and velocity of the circulation.

The reasoning on which this explanation rests is succinctly put by Dr. Murray: "The animal temperature," says he, "is derived from the consumption of oxygen gas by respiration; and an increase in that consumption will occasion a greater evolution of caloric in the system, and consequently an increase of temperature; while a diminution in the consumption of oxygen will have an opposite effect. If then, when the temperature of the body is morbidly increased, we introduce into the stomach substances containing a large proportion of oxygen, especially in a loose state of combination, we may succeed in reducing the morbid heat."* Such, is a brief outline of the explanation which chemistry offers, of the action of those medicines which have long been admitted to exert a control over the temperature of the body when morbidly increased; and along with this, a control likewise over the actions of the heart and lungs. The theory is doubtless open to many objections, chiefly owing to the somewhat unsettled state of our knowledge as to the true source of animal heat, and the connexion which exists between it and the actions of life; but when we take into account that all the remedies which for centuries have been admitted to exert a refrigerating influence, are in fact such as contain oxygen in great abundance; we can scarcely fail to conclude, that even in a body structurally healthy, there exists a power of receiving and applying that element in such a manner, as to diminish the necessity of its reception by the lungs.

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* Murray's Elements of Chemistry, vol. 1, p. 494.

But if such a faculty be admitted to exist in a body, whose structure is as yet normal, and if advantage may be taken of it in the treatment of diseases which depend on functional changes alone, still more may it be supposed to present to us a probable advantage, where, from alteration of structure in the lungs, the functional powers of these organs are impaired, and the proper balance between the producing and consuming organs of arterial blood subverted; and it is perhaps on this principle we may explain, how acids, nitre, and the like remedies, which have been, on high authority, asserted to prove useful in consumption, act. Without attempting to deny that this is often the case, or that much seeming benefit may result from the use of acids in advanced stages, where the prominent symptoms are dependant on tubercles which already exist, yet it will clearly be inferred from all which has before been said, that they are not remedies to which I should be disposed to attribute any high degree of importance. For conceiving, as I do, that the only possible prospect of a cure in phthisis depends on arresting the deposition of tubercular matter in its advance; and feeling convinced that we possess at present but one known remedy which can effect this purpose; it follows necessarily, that we ought not to sacrifice the *major* to the *minor* intention; and so, without questioning the efficacy of acids in ameliorating the symptoms of reactive phthisis, I object to their use, because they unavoidably interfere with means essential to the treatment of the disease in its formative stage. Independent however of this objection, it is admitted on all hands that the power of refrigerating remedies is not great, and indeed, even assuming the chemical explanation of their action to be correct, it is scarcely to be expected, that through the stomach alone, we should be able to introduce an amount of oxygen sufficiently large to produce any marked effect. The principle, however, appears susceptible of an application more extended, and one which is practically of more importance. If it be true that the stomach, either by decomposing substances which contain oxygen, or by absorbing this element when presented to it in an uncombined state, be capable of charging the blood with this gas, it is not less probable, that surfaces analogous in structure and function to its lining membrane, should be capable of effecting the same purpose; and as the facility with which this power can be called into action, and the extent of surface on which it can be executed, are much greater in the one case than in the other; so it does not appear irrational to suppose, that the same vicarious oxygenation which relieves the lung through the intervention of the stomach, might relieve it to a greater extent, if executed by the skin or other membranes.

That the contact of atmospheric air with the skin of cold-blooded animals, is sufficient within certain limits, to sustain life by the induction of changes in the blood analogous to those of respiration, had been long ago shown by Spalanzani, and since confirmed by the experiments of Dr. M. Edwards. The same absorption of oxygen, and the same production of carbonic acid, takes place in the one case as in the other; and life is sustained for a considerable period, even when the lungs are entirely removed.

Some experiments of Mr. Abernethy go to show, that a similar action takes place on the surface of the human body; but the extent to which this proceeds, or the amount of influence it exerts on life, is not, by them, indicated. There are practical difficulties connected with this investigation as regards the skin, which are not readily overcome: it is no easy matter to retain a gas of any kind, in contact with the surface of the body, for a sufficient period to insure its action, and at the same time to admit of examination afterwards. I have made the attempt however many times, employing pure oxygen, as any change wrought on it would be more readily perceived than when atmospheric air only was used. An air-tight cylindrical bag, narrow at each extremity, but wider in the middle, and there provided with a small stop-cock, was slipped over the arm, and the two ends bound closely down with repeated bands of adhesive strapping; the air contained in the cylinder was then expelled through the stop-cock, as much as possible, by applying the flaccid bag closely to the skin, and its orifice closed. By then inflating the bag with oxygen gas, such portion of the arm as intervenes between its two extremities is effectually surrounded by this air, and may be retained so for any length of time: after this arrangement has remained for a period—which in my own experiments has varied from three to eight hours, the included air when examined in Hope's Eudiometer, by the tests of lime-water, or caustic potash, always exhibited the presence of a large though variable proportion of carbonic acid, the residue being oxygen unchanged, as shewn by the action of nitric oxide gas. The quantity of carbonic acid thus produced, sometimes amounted to a fourth—sometimes to a third of the entire volume, but never exceeded this; its increase appearing to depend on the length of time for which the contact of the oxygen and skin was continued. There seemed also to be a difference in this respect connected with the fluctuating activity of cutaneous circulation at the times when the experiments were tried.

These observations, though I am aware, wanting in precision and number, go to establish the leading fact, that free oxygen applied to the cutaneous tissue, undergoes a change analogous to that which it sustains in the lungs, and therefore confirm the conclusions arrived at by previous inquirers, that blood undergoes to a certain extent, when circulating on the surface of the body, changes similar to such as are effected on it, to a much greater extent, in the pulmonary apparatus; or, in other terms prove that, within certain limits, the skin is to be viewed in man as a respiratory surface.

But if this be so as regards the skin, still more should we expect it to occur on internal surfaces; the mucous and serous tissues more especially. This is without any question true, and proofs of it are more easily acquired, because experiments which attest the fact are more easily performed. Let us first refer to the gases very generally—I might say, constantly found in the alimentary canal of dead persons. It may be set down as a universal truth, at all events as a very general one, that free oxygen is not found in such situations: I have myself examined air taken from the intestines in twenty-seven cases, indiscriminately, and without regard to the diseases of which the parties died, by the test of nitric oxide; and in no example have I found it, with one exception, and in it there seemed every reason to suspect that atmospheric air had by an accident been admitted: all contained a large quantity of carbonic acid, mixed often with hydrogen, and the gases formed by its union with carbon or sulphur; and frequently with a residual air, which from the negative qualities it presented, I inferred to be nitrogen. It may however be objected to this, that as these gases without doubt are frequently evolved in the intestines themselves, from the chemical re-action of their contents, that it is not probable free oxygen should be found in them. This objection I could readily admit in a few isolated cases, but when we come to deal with a considerable number, and consider that atmospheric air largely enters the stomach by its union with the food and drink, not to mention that it is also frequently swallowed, I think we are justified in concluding, that its vital element would be occasionally found in the canal, unless its properties were altered during progress through it.

The power of the mucous membrane in changing oxygen is more certainly shown by direct experiments: twelve cubic inches of pure oxygen gas were injected into the rectum of a large rabbit, and measures adopted to prevent its escape; the animal was killed in three hours, and the intestines examined. The rectum and a portion of the colon were highly inflated; of the gas which they contained, about a third was absorbed by lime-water, the residue showed distinct traces of free oxygen, blended with an air which appeared to be nitrogen, the source of which is not very evident. The blood vessels running along the mysentery from those portions which contained the gas, were charged with florid blood, and the mucous tissue of that part was of a much redder colour than in the upper portions of the canal. Being anxious to see how far death by asphyxia would increase the absorption of oxygen, I varied the experiment in the following manner. About the same quantity of this gas was thrown into the rectum of another powerful rabbit, and the same precautions used against its escape: the animal was then included under a glass jar capable of containing nineteen imperial pints of air, whose edges were surrounded by water so as to prevent either the escape of that within, or the entrance of fresh air from without. The animal died asphyxiated in eighty minutes from the time of enclosure, and the body was examined a few hours afterwards. The large intestines contained a considerable quantity of gas, about one-third of which was absorbed by lime-water—the air, both as taken from the animal, and after being deprived of its carbonic acid, extinguished a lighted taper, and showed only the faintest trace of red fumes when tested by nitric oxide. The oxygen therefore had very nearly disappeared; but here, as in the last case, a considerable portion of gas remained, the nature of which I was not able to determine. The minute vessels of the mysentery, and those of the lining membrane of the canal, where the gas had penetrated, were filled with blood of a florid red appearance. It would seem probable from this trial, that the absorption of oxygen by the mucous surface of the intestinal canal, is at all events more speedily, if not more effectually accomplished, when the animal is deprived of the means of receiving the necessary gas by the lungs; for in the first experiment, the oxygen remained in contact with the membrane for three hours, during the life of the animal, while in the second, it remained only eighty minutes; yet the changes it underwent, and the influence it exerted in reference to the living parts, were even more marked in this than in the former trial. The fact of the absorption of oxygen in this manner being admitted, it seems legitimate to infer that, it must exert some physiological influence on the general mass of blood, and so far as the quantity taken in goes, diminish the necessity of action by the lung: on this supposition we should be led to expect that the period of time at which full asphyxia would be induced on an animal, situated as in the above experiment, would be retarded by the presence of oxygen in its intestines. I made many experiments with the hope of obtaining direct proof of this, employing rabbits of as nearly as possible the same strength, size, and age; but they led to no useful result; the period of death in a given quantity of air appeared to be little influenced by the absence or presence of intestinal oxygen. This, however, is what might reasonably be expected, when we consider the exceeding small portion of this gas, in comparison with its aggregate consumption, which can in such a way be experimentally employed; but it does not in any manner detract from the leading fact of its cutaneous or membranous absorption, or invalidate the conclusion, that this action if exerted over extended surfaces, and with much activity, must in all probability influence the general mass of circulating blood, and at least in some degree diminish the necessity for its pulmonary change.

The facts which I have now adduced will be perhaps considered sufficient to establish, that free oxygen, placed in contact with the skin, and other living textures, is influenced in the same manner as it is in the respiratory organs; and the inference fairly follows, that the blood flowing on these surfaces is reciprocally influenced also to a similar extent, or we may conclude, that the external surface, as well as the internal lining of cavities, perform a function analogous to that of respiration, even during health, wherever oxygen finds admission to them.

Viewing this then as a vicarious action, always exerted to a certain extent, it may not appear an improbable supposition, that it will be performed in a higher degree, where the execution of the lung's function is from any cause diminished, and that it will be especially liable to increase in phthisis, where there seems reason to believe, that the arteries are supplied with a blood imperfectly aërated; and the resulting question appears therefore to be, not as to the possibility of systemic aëration, but as to how far we are able, by any artificial arrangements, to render this available in practice. I here pass over any consideration of those substances which have been occasionally supposed to act, by introducing oxygen in a combined form through the stomach. Of all such, acids are those from which we should expect the greatest benefit, when given with such intention, and I have already at p. 305 expressed my reasons for considering their employment quite contradictory to the fundamental practice, which, in consumption, constitutes our best and only sure resource. To the neutral salts, more especially nitre, which have often been recommended, with a somewhat vague reference to the same principle, this objection does not apply; but independent of the circumstance that their powers are admittedly but small, most of them possess the quality of acting on the bowels, and irritate their mucous surfaces in a manner highly detrimental to the general interests of the patient. To engage the surfaces of the stomach and bowels therefore successfully, as organs capable of introducing oxygen, the more safe and probable way would seem to be, either by direct application in a free state, or at least in combinations, which did not interfere with the fulfilment of other and more important indications. It unfortunately happens, that oxygen can be made to combine with water in small quantities only, even when the highest pressure is employed, so that the amount introduced under the form of a really oxygenated drink, must always be trifling, and therefore ineffectual. The gas however forms other compounds—the nitrous oxide especially, in which it exists in a very loose state of combination, and which water very readily absorbs; and, impelled by physiological considerations not dissimilar to those which it has been my object to enforce, Mr. Searle, an ingenious gentleman, applied this idea practically, some years ago, under the conviction, that an air, whose effects are so well known, when received into the lungs might form a valuable auxiliary in many diseases, when applied to the stomach under the form of an aërated water. This fluid, under the ordinary pressure of the atmosphere, and at common temperatures, dissolves about its own volume of the gas, but by increased pressure, Mr. Searle has practically shown that five volumes may be absorbed, and that it thus forms an effervescent drink, similar in appearance though not in effects to soda water and other solutions of carbonic acid. I have had an opportunity of subjecting this water to the test of experiment on various occasions, both of health and of disease, and the effects which result, as regards its influence on the circulation, as well as breathing, are somewhat curious. The gas, when inhaled, has been always stated to exert a stimulating influence, exciting the actions of the heart, and augmenting the energy of the brain and nervous system, and we should therefore be justified in the supposition, that when received into the stomach it would induce similar effects, if indeed any effects at all were perceptible. This however is not the case, as we shall yet see. I pass over some speculative views of the inventor, which however ingenious in themselves, are unsuited to our present purpose; but one leading idea of that gentleman seems to have been, that the compound acts as a stimulant to living power, by communicating oxygen. Mr. Searle's attention was however, I believe accidentally, attracted to the fact, that in some respects the very reverse of stimulation resulted; that the water reduced the action of the heart, instead of augmenting it, and thus appeared to exert even a sedative influence. This effect seemed so illustrative of opinions I had long held, that I felt desirous of testing its truth by experiment in my own person.

Of six trials made with this object, all of which led to very similar results, a report of two only may here suffice.

At half-past ten in the morning my pulse numbered seventy-four beats, and my breathing twenty respirations in the minute. Between that hour and eleven, I had drank, in divided quantities, the contents of two bottles* without any change in the pulse or respiration being perceptible: by half-past eleven, one hour from the commencement of the experiment, I had taken half a bottle more; the pulse then numbered seventy, the respirations were seventeen in the minute, the action of the kidney was increased; the remaining contents of the bottle were then taken: at twelve o'clock the pulse numbered sixty-six, and the respirations sixteen; there was a feeling of distension in the stomach which soon went off, and the kidneys acted strongly; at four o'clock on the same day

* Each bottle contained ten fluid ounces.

(having been employed during the interval in ordinary occupations), the pulse had risen to seventy-eight, and the influence of the medicine entirely passed away.

Some few days after the last trial I drank the contents of two bottles within the hour; my pulse at the beginning numbered seventy-five, and the respirations nineteen in the minute. In a quarter of an hour afterwards the pulse was sixty-eight, and the respirations seventeen; similar results, as I have already said, were obtained in four other similar experiments. In the two here set down, the pulse in one, fell eight beats, and the respirations four, on taking the contents of three bottles within an hour and a half: in another the pulse was diminished seven beats, and the respirations, two in the minute after two bottles full had been employed within one hour.

The fact, then, that this aqueous solution of a gas which contains oxygen in abundance, and in a loose state of combination, does reduce the pulse after its reception by the stomach, appears tolerably certain; but the question occurs, why this reduction takes place? that the gas is really absorbed, cannot I think admit of doubt, for otherwise, the quantity I have described myself as swallowing would surely have led to eructation, as carbonic acid does; yet this was not the case, and I have not seen it in other examples, where however it was less lavishly employed.

It has been objected to these experiments, that the reduction in the pulse may have arisen, from the impression of cold made on the stomach, and thence propagated by a sympathetic re-action to the heart. I believe the objection to be untenable; at least, I have never found any very appreciable influence exerted on the pulse by the same quantity of cold water, swallowed in the same time, while, as already seen, the solution of nitrous oxide always produced a marked effect on it; and I think we are in consequence bound to suppose that this arises, from some relation which exists between living power, and the chemical compound used; and further, to conclude on reasonable grounds, that such relation depends on its partially changing the blood on systemic surfaces, and thus diminishing the necessity, to a certain extent, for the consumption of oxygen by the lung.

But if it be probable that its effects on the heart and breathing, during health, be attributable to such a principle of action, so it seems also probable that it will be capable of relieving several distressing symptoms, more especially dyspnœa, which are often present in old chronic diseases of the thoracic viscera; or that, if employed in a steady and persevering manner, where disease is not yet hopeless, it might, in concert with other appropriate treatment, conduce to even permanent advantage.

My opportunities of employing the remedy in disease, and more particularly in phthisis, have not been extensive, chiefly owing to its price rendering it inadmissible to any extent among the patients of public charities, from whom my chief experience of the disease has been acquired. In seven examples only have I used it, and those were persons afflicted with severe paroxysms of dyspnœa, arising from long standing affections of the lungs or heart. Of these, three were cases of consumption in its latest stages; two were examples of diseased heart; and in two the breathlessness appeared to depend on emphysema, induced by long existing bronchitis.

In all of these, the breathing received decided though temporary relief; and that this was truly the case, and not imaginative merely, appeared certain, from the pulsations of the heart, and the acts of respiration being numerically lessened, to some extent in all. The inventor has published several cases, under the authority of gentlemen well known to the profession, who have arrived at similar conclusions.

I ought perhaps to offer an apology, for dwelling on this apparently trivial point so long; I have done so from feeling, that if there indeed be any truth in the principle urged throughout this section, we possess no more probable means of augmenting the living action on which it depends, than that presented by the nitrous oxide; and this, more especially in phthisis, where the use of acids, as I have often before stated, is in my belief altogether inadmissible. But in prosecution of the general question, of vicarious aëration of the blood, there are probably no substances which can be so frequently and in such quantity, brought into contact with the lining membrane of the stomach, as to justify a hope that decided permanent benefit can be derived from them; and we must hence look to some other surface on which the process naturally proceeds, with a view of endeavouring to augment this as much as possible by every means within our power. Such a surface is without doubt presented by the skin, and its proper management becomes on this, as well as on all other accounts, a point of great importance in the management of formed or re-active phthisis.

I have before added my mite of proof, to such as were long ago given by Abernethy and others, that the surface of the body acts on oxygen brought into contact with it, in a manner similar to the lungs, or in other words that this is to be viewed, to a certain extent, as a respiratory organ. My own attempts to determine the point were made with pure oxygen, and the effects, perhaps, were more decided than when atmospheric air only is employed; but many previous ones have been made with air alone, which offers a closer analogy to the process, presumed to be constantly proceeding on the cutaneous texture; and when we consider how extensive this is, and how constantly it is placed under the external circumstances which favour such an action, it is surely not drawing a very wild or fanciful conclusion, that it may, during the continuance of pulmonary disease, aid the impaired lungs to some extent in the execution of their functions. This being the case then, there are two points to which attention should always be directed, with a view to the encouragement or increase of the action, which is here presumed to have existence. The first, is to retain the skin in a state as free as possible from any adherent matter which might impede absorption from without, and by all means to stimulate the cutaneous vessels, that as much blood as possible may be brought into close vicinity with the air. The second is to adopt all measures not incompatible with other indications, which may tend to change the air which surrounds the body of the patient; for unless this be done, there seems every reason to believe that a layer of carbonic acid frequently envelops it on all sides, more especially where covered with the clothes, and thus greatly obstructs the process it is our object to encourage.

The modes of carrying out the first of these intentions are sufficiently obvious, and resolve themselves simply into a strict regard to the cleansing of the skin, and the increasing of its circulation by gentle frictions, or other means which gain the same end. To effect such purposes the patient's body should be carefully washed with soap and water, or a very dilute solution of soda, not less frequently than twice a week, and rubbed perfectly dry afterwards with a coarse but soft towel. On the intermediate days the process of dry friction should be adopted to an extent, which must be greatly regulated by the cutaneous irritability in each case; sometimes the towel only can be borne with comfort, but where the tenderness admits, a soft flesh brush attains the end we desire in a more effectual manner. In cases where the circulation of the surface is vigorous, and the patient's powers not as yet greatly impaired, frictions with diluted distilled vinegar, carefully dried up afterwards by strong friction, I have often found decidedly beneficial, greatly conducing to the immediate comfort, and at times to the permanent benefit of the invalid. Of these means of treatment, none can be considered as of a novel character, though the principle on which I presume their benefits to depend, is not the one commonly recognized. They have usually been explained on that of revulsion or derivation. It has been theoretically supposed, that for every portion of blood which can be directed *to* the cutaneous vessels, an equivalent portion is withdrawn *from* the lungs, and those organs consequently relieved. Such a doctrine cannot be supported by an appeal to physiology; so far from this, it is easy to show that the very reverse must be the case. Increased action of the cutaneous as of all other vessels, implies activity—not stagnation; indicates, not, that they have received and retained in them more blood than before; but, that they have transmitted through them an augmented quantity in a given time, on which is effected during its transit, a change, the converse of that which it must of necessity again be subject to in the lungs; and hence it would appear, that the action of these organs must be increased by the very revulsion which is presumed to diminish it. This position, I believe, cannot be controverted; and admitting fully in common with many others the general benefits of a treatment similar to that described, I submit, that the usual explanation is not satisfactory: whether the one I have attempted to substitute be tenable or not, each reader will of course determine for himself; for the advantages which result from its practical application I can fully answer.

Having by these means prepared and retained the skin in a condition best fitted to favour the action of atmospheric air, the next most important end to be accomplished is, that the air brought into contact with it should be constantly renewed. This is of course gained by any circumstances which create motion in the air itself, or which transfer the patient with a certain degree of rapidity through air which is at rest. The first cannot on many accounts be rendered available to any great extent: the last we have very much at our command.

Riding on horseback or in a carriage, walking, running, sailing, the use of the swing, and several others, obviously accomplish it, and the important question is, to select from amongst them. When before (at p. 283) discussing the principles which should guide us in the choice of different exercises, I offered my reasons for objecting to those which involved much of muscular effort on the part of the patient, and arrived at the conclusion, that whether considered theoretically, or in regard to practical results, swinging and sailing were preferable to all others. I may now add, that the cause of this superiority appears to depend on the continual change of the air which surrounds the body, and the accomplishment of this, independent of that additional activity in the actions of the lungs and heart, which muscular contraction at all times involves. Beyond this I have little more to say on the subject, except to offer a few suggestions which may be of some practical utility in their application.

The purer the air in which these exercises are taken, of course, the better; and the temperature should, if possible, not be lower than sixty. Sailing, except in fine and temperate weather, if performed in an open boat, or under free exposure to the atmosphere, as it always ought to be, is clearly inadmissible; the evils of cold, or wet, being greater than the benefits derivable from its employment. Sir James Clark has very properly remarked, that to the unimpeded facility and safety with which exercise in the open air may constantly be taken, we must attribute a large portion of the advantages arising from residence in a climate more equable and certain than our own; and to no mode of exercise does this apply with greater truth than to that of sailing; for, we are unfortunately much restricted in its use, by the uncertain variable character of our own atmosphere: still there are many days—nay, months during each year, which safely admit its application; and these, when circumstances admit, should be assiduously employed.

The analogous exercise of swinging is luckily much more manageable, because shelter, at least from wet, can always be commanded; when the weather and temperature permit, it should be performed in the open air—when rain alone creates a difficulty, a barn or other similar building, protected by a roof, but open at the sides, is the best situation; and when the temperature of the air is under sixty, or even at that, if the decline has been sudden, the interior of a well-ventilated house should be selected. These are all points of adaptation which can here be treated of in general terms only.

Before concluding this section, I could desire to say a few words on its contents. That to many, these will appear in the highest degree speculative, and the proofs of its positions extremely imperfect, no man will be more ready to admit than myself. That the theoretical views which it contains may prove *suggestive* only, is perhaps the highest hope I am entitled to indulge; and should they do so, it may not have been written in vain. An idea has been afloat in our art for many years, that no doctrine should be received, which is not based on the same demonstrative evidence which physical science admits: there never, perhaps, existed a greater self-deception; and men who hold the belief are perhaps, after all, the greatest theorists. Even the strenuous adherents of morbid anatomy —a department of medicine, which, without question, approaches most nearly to precision, but which by a curious misnomer, is often substituted for general pathology, of which it is clearly only a department—forget, too frequently, while investigating physical effects, to dwell for one moment on the vital changes which produced them. That a rapid change is in this respect coming over the "spirit" of the medical "dream," is at least probable; and while I believe and hope that it will ultimately tend to establish more expanded views of vital pathology, I have here felt myself entitled to theorise in some measure on the causes of those benefits, which empiricism itself has long admitted to spring, from certain modes of practice applied to a malady, whose obscurity we all lament.

SECTION III.

Local Treatment by Inhalation.

By the term, local treatment of phthisis, two things may be expressed; we may include under it remedies applied to the parietes of the chest, with a view of counteracting the disease proceeding within its cavity; such as counter irritants of all kinds, leeches, cupping glasses, and the like; or we may restrict it so as to include those remedies alone, which in the form of airs, or vapours, are applied by inhalation to the interior of the diseased lung itself, with various intentions.

I shall in this section speak only of the latter class of substances, reserving the former to be noticed in the next, where the complications of consumption are discussed; and I adopt this division from being impressed with a strong conviction, that it is under such circumstances they alone are useful, and possess little if any control either over the action by which tubercles are produced, or the symptoms they afterwards originate, except, in so far as these depend on secondary irritation, induced by their presence.

No system of treatment was more likely to suggest itself to the minds of physicians in consumption, than the application of remedies in a vaporous or aërial form to the diseased lung. The inspiratory act renders their reception easily effected; the substances inhaled are applied directly to the textures involved; and so long as men looked to the local ulceration only, without attempting to unravel the constitutional changes which produced it, the analogy appeared perfect between the healing of external ulcers under various applications, and that of pulmonary ulceration by a similar method. We accordingly find that the Greek and Arabian writers furnish us with many advocates of the practice: that after the revival of literature in Europe it was somewhat lavishly employed; and that to approach more nearly to our own times, it has been highly extolled by Bennet, Mead, and a variety of others. Of late years, after suffering an apparent suspension, it has been again revived, and now continues to attract some attention on the Continent, as well as among ourselves.

The history of inhalation may fairly be considered as comprising three eras. It originated at a time when the existence of oxygen was not suspected, and the true theory of respiration as a necessary consequence unknown. The only object in its application therefore, was to heal ulcers of the lung as ulcers of other parts are healed; and we hence find, that stimulants of all kinds—the salts of mercury and arsenic—fumes of sulphur—turpentines—and balsams of every variety were the substances constantly recommended and employed.

The advance of pneumatic chemistry led to other views. The discovery of oxygen by Lavoisier towards the latter end of last century, which throughout revolutionized chemical science, exerted a powerful influence on the doctrines of physiology; and when it was found that on the reciprocal action between this gas and the blood, the whole phenomena of life immediately depended, nothing could be more natural than to conclude, that errors in the action of the lungs might be advantageously met, by supplying those organs with an increased quantity of the element to which they owed their efficacy during health. That such a conclusion was indeed fallacious, experience seems to have decided; and when the time arrives it may not be difficult to determine on what circumstance this depends. But independent of oxygen, various other gases were also identified about the same period, several of which appeared also to offer probable resources in diseases of the lung; and it was hence, though with different intentions, that the inhalation of hydrogen and its compounds, were from time to time recommended in consumption as powerful sedatives. In the one case oxygen was used, on the ground of its directly supplying the system with a substance essential to its vitality; in the other, hydrogen was recommended, on the belief that it diminished the actions of life, and thus benefited the irritative fever which was found to prevail. The third and last of our assumed eras brings us down to the present time. Chemistry had greatly advanced, many new substances been discovered, or the constitution of old ones clearly indicated; amongst others, chlorine, hydrocyanic acid, and iodine—the acid was found to act as a powerful sedative, the iodine as a strong stimulant of the absorbent system, and the other stated to possess virtues, not distinctly referrible to any known principle of action. On these various grounds, sometimes empirically only—sometimes for the purpose of fulfilling particular intentions—they have one and all been employed for the purpose of inhalation.

We thus find scattered over the records of medicine, a very large number of heterogeneous substances, differing in their qualities and therapeutical powers, recommended as fitting agents for direct application to consumptive lungs, and we must, I conceive, be driven to conclude, that all cannot have been proper, provided we view the plan as one calculated to exert a control over the disease by which these organs are occupied. But as the malady is extremely variable and complex in its nature, presenting symptoms in one subject altogether distinct from those which it presents in others, so it is not improbable that each remedy may occasionally have exerted a certain amount of sanative influence, in particular cases, and under particular conditions; and it would certainly be desirable that the circumstances under which each may be proper were in some measure pointed out, always holding in remembrance that in *no* example must we hope for a cure of phthisis from *any* local application ; or rationally expect that it can accomplish more, than feebly to co-operate with the constitutional treatment, on which our hope of final success must be ever based.

We may perhaps arrive at something like intelligible principles for our guidance, by arranging the substances which have been employed by inhalation into four classes :—

The first may contain that which has been supposed to act by presenting to the lungs, in a concentrated form, the element on which their action depends—oxygen gas itself.

In the second class may be arranged those remedies which have been presumed to diminish pulmonic irritability—narcotics, hydrocyanic acid, and hydrogen.

The third may include such medicines as, by stimulating the surface of ulcers or excavations which exist in the substance of the organ, may be presumed capable of inducing in them a new and healthier action—metallic fumes, sulphur, tar, the various turpentines, and balsams of all kinds.

In the fourth class we may place two new bodies which have of late years been empirically employed, namely—iodine and chlorine.

1. I have hinted before, that the advantages theoretically supposed to arise from the inhalation of pure oxygen, or of this in a more concentrated form than is found in atmospheric air, appears to proceed on a false hypothesis. We are fairly entitled to conclude that the natural adaptation of air to living creatures, is such as to present their vital element in that state of dilution which is best suited to their wants; this, in round numbers, is about one of oxygen to four of nitrogen. When the compound has been breathed, about one-third of the first of these gases is found to be withdrawn, but in the residual air, enough still remains to support life; and it is only after this has been passed many times through the lungs, that it becomes no longer capable of effecting such purpose. The broad fact then which here meets us is this, that atmospheric air contains a larger quantity of oxygen than the lungs are capable of at once applying, and the

inference follows, that when these organs are diseased, the resulting evils do not depend on a want of their external excitant, but on the absence of that organic or functional integrity which admit of their applying it in a natural and healthy manner; and it is hence not by augmenting the quantity that we can fairly expect any benefits to arise. Experience seems to establish the truth of this conclusion. When oxygen is inhaled in a pure state, it is found always to exert a stimulating influence prejudicial to the patient, producing an increase of heat and irritation, exciting the actions of the heart and lungs, and even congestion or inflammation of these organs. When again it is used in such quantities only as merely increases the purity of the air breathed, little or no advantage is attained—its effects are either negative, or at most it acts as a slight local stimulant, without in any way augmenting the facility with which respiration is performed. For these reasons its use as an inhaled gas has long been exploded, and does not seem likely to be soon revived.

2. Sometimes in the early stage of tubercular deposition, and more frequently when partial softening has taken place, a considerable degree of local uneasiness and irritation prevails. The character of the pain is generally not acute, but more a sense of constriction and oppression diffused over the chest generally, but especially felt at its upper portions. The cause of this appears to me to depend on one of two conditions, either a certain

amount of spasmodic constriction in the smaller bronchi, or a low chronic form of inflammatory action induced by the tubercles, and occupying either the lining membrane of the passages, or the true breathing tissues of the lung. When these symptoms are not developed with such intensity as to demand more active treatment—as is frequently the case—the inhalation of anodynes is often attended with considerable advantage. It is probable that many medicines of this class might be employed with equally beneficial results, but of two only have I any personal experience—opium and conium. They may either be used in the form of tincture, mingled at the time of use with the hot water whose vapour forms the medium of their transmission, or employed by infusing the same ingredients in water only, at a high temperature; by this method we procure a simple watery solution of their active principles, and avoid the irritation which alcohol, otherwise intermixed, is occasionally liable to produce. When we have reason to apprehend the presence of spasm, the addition of a small quantity of sulphuric æther, is often beneficial. It is proper to observe, that, for the suggestion of conium, as here applied, we are indebted to Sir Chas. Scudamore, whose experience of inhalation appears to have been considerable, and his estimate of its advantages high. The benefits of hydrocyanic acid, a remedy whose inhalation has been said to produce very beneficial effects, may probably depend as much on its inducing a general sedative effect after absorption, as on any local influence it exerts. I have never seen it of much advantage, possibly because I have never carried its application sufficiently far. Of its powers as a sedative, however, no two opinions can be held, but it unfortunately happens to be like digitalis, a very uncertain, and consequently unmanageable medicine. Employed in large doses, either by the stomach or lungs, we are never sure that its action may not extend to a point beyond that of perfect safety, while if very moderate ones are used, its effects are often inappreciable. The only remaining substance whose sedative powers have been appealed to as a probable means of allaying irritation of the lungs, by direct application, is hydrogen gas. It has however been little tried, and that little has not led to advantages sufficiently great to encourage further perseverance. Dr. Beddoes alone mentions a case, in which a young lady, under most unpromising symptoms, was cured by inhaling, twice a day, a quart of the gas mixed with fifteen of common air: he hints too, that the benefits of residing in a cow-house may depend on the intermixture of hydrogen or its compounds, with the atmosphere it contains. A few cases of consumption improved or cured by employment in coal pits, have been also adduced, as showing the advantages derivable from this gas, but on the whole the evidence in its favour is far from conclusive.

3. But while anodyne and sedative inhalations have thus had their advocates to some extent, a

very different class of substances, and applied with very different intentions, have been recommended by others. Such are contained under the third head of our arrangement, and must be supposed to act by the local stimulus they communicate, either to the surface of the membrane which lines the bronchi, or to the surfaces of ulcers or excavations which occupy the lungs.

A practice having this object in view dates from a very early period—so early as the time of Galen; the remedies then employed were chiefly the fumes of arsenic or orpiment in a state of high dilution; or these, united with the vapours of different balsams, and resinous substances, such as storax, myrrh, galbanum, and the like. The metallic fumes do not appear long to have maintained their reputation, and even resinous and balsamic appliances, after being lauded by Bennet, Mead, and other physicians of the seventeenth century, had nearly sunk into oblivion, when the practice—at least one very analogous—was again brought into notice about twenty years ago by Sir Alexander Crichton, in his work on the uses of tar vapour.* It will probably be admitted, that whatever benefits may have been derived from the various resinous, terebinthinate, or balsamic remedies, would be equally derivable from this more recent application;

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* Practical Observations on the Treatment and Cure of several Varieties of Pulmonary Consumption, and on the Effects of the Vapour of boiling Tar in that Disease, by Sir Alexander Crichton, M.D. F.R.S. &c. &c. London, 1823.

that the principle on which they act, must be nearly, if not entirely, the same; and that a few brief remarks, on it only, may therefore serve for all. The method of administering the remedy which the author advises, is perhaps the best which can be suggested for all substances destined to be received by inhalation, the charging the air of an apartment with the vapour to be used. A vessel of common tar is in this instance placed in the desired situation, which, on application of heat, evolves the vapour, so that any required degree of impregnation may be communicated to the air around.* The author's experience in Russia, where the plan was first tried, led him to conclude that many cases of true tuberculous consumption had been cured or relieved by its adoption, while his success in bronchitis was, as might naturally have been expected, still more decided. Among ourselves, experience has, unfortunately, not exactly confirmed the sanguine hopes of the suggestor, and there are few, I believe, who will now hold that his plan exerts much control over those conditions of the lung which decidedly own a tuberculous origin. The case however appears to be different in examples of purely chronic bronchitis; there seems a stage in that disease, where, all inflammatory symptoms having subsided, the distress is dependant in a great measure on exaggerated secretion of the bronchial membrane, arising from want of tone in the exhalent vessels—or excretory ducts of the mucous follicles, and when these are invigorated there seems every reason to think that great benefit results. This end, the tar vapour judiciously employed, in concert with appropriate internal treatment, seems frequently to gain, and indeed there appears ground for the suspicion, that some at least of the reported cures of phthisis, by this remedy, have been in truth cases of the other affection.

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* Dr. Paris has suggested a useful chemical addition. Common tar contains a free acid, whose fumes are acrid; by mixing a portion of carbonate of soda or potash with the tar before use, this acid is combined, and the tar vapour in a pure state evolved.

4. The last three classes contain remedies applied directly to the lungs, with some definite intention. Under the present I shall briefly advert to two others, whose introduction into practice is of a recent date; and which, it seems more expedient to consider, simply in reference to the opinions which have been formed of them by practitioners. Chlorine, the first of these, was suggested about fifteen years ago by M. Gannal, a French calico printer, from remarking, that some consumptive workmen appeared to be relieved, while in the course of their avocations they breathed an atmosphere charged with this gas. He presented several memoirs on the subject to the Academy of Medicine, which led to more extended trials, and the powers of the remedy have now been so long tested, as to admit of our forming some judgment of its actual merits. It would not appear that general experience has pronounced any more

favourable verdict in regard to chlorine, than it had often before pronounced of other medicines, employed in the same manner. In many trials which I have myself made, it appeared merely to act as a powerful local stimulant, and to be admissible only in chronic cases, where irritation or excitement of the lungs was absent. In all others it invariably induced symptoms of greater severity than those it was intended to relieve ; and even in cases the best adapted to its use, the advantages which occasionally appeared to result were always temporary and deceptive. It is right however to add, that a much higher estimate of its powers has been formed by others. The most decided example of benefit arising from its use, is reported by Dr. Cattereau, in a French journal, and copied into the Lancet for 1830-31. The patient who, as dissection ultimately proved, had suffered from tubercular excavations of the lung, appeared to have them healed under the influence of the inhalation. Dr. Elliotson is disposed to think favourably of its powers in alleviating, though not probably in curing the disease, and adduces several cases in support of this opinion ;* and Sir James Clark attests, that in several cases it relieved dyspnœa, and in some allayed the cough, but that "in the majority it produced no sensible amelioration."† This is qualified praise, praise that might with equal justice be bestowed on a host of other remedies, and on the whole, there seems but small reason to hope that the introduction of chlorine has much enlarged our resources in the treatment of consumption.

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* Vide Clinical Lectures, by Dr. Elliotson, *Lancet*, vol. 2, 1830-31, p. 198.

† Vide his Work on Consumption, p. 372.

The known powers of iodine over the absorbent system, may naturally have suggested it as a promising remedy when applied directly to the lung, but I am unable to determine by whom its use in this way was first attempted. Sir C. Scudamore published a short treatise on the subject in 1830. Dr. Elliotson, in the same year, reports his experience on the question in his Clinical lectures; and it has since been tried to some extent, by many others. The first of these physicians is now the principal advocate of the practice in this country, and has from time to time, favoured the public with so many cases of its success, as to justify the conclusion that, under proper circumstances, and when dexterously employed, it is not without its merits. Certain however it is, that the same beneficial results have not been obtained by others. Dr. Elliotson appears to doubt exceedingly whether it possesses any curative power over true tubercular phthisis, and even seems to question its influence in alleviating symptoms. My own experience on the point is very slender, but so far as it extends, the estimate of Dr. Elliotson has appeared correct. One thing is certain, that its action is more irritative than chlorine; it hence demands greater care in the administration than even that gas, and it is probably on this account that we find Sir C. Scudamore generally combining it in practice with a saturated tincture of the leaves of conium. Even this precaution, however, does not always render it a remedy of very easy adaptation. Where there exists any irritability of the larynx, or trachea, or any inflammatory tendency about the respiratory organs, the propriety of employing it is extremely questionable, and on the whole it demands more caution than any similar application which is employed in the same way.

I have briefly noticed the principal remedies employed in consumption, as direct applications to the lung. That the method is sometimes useful cannot be questioned, but it should ever be considered as of secondary importance to the constitutional treatment we may deem proper; for, he who looks on phthisis as a local malady, and remediable by local means, will assuredly find himself mistaken. Neither is it to be forgotten, that if inhalation at times confers benefit, it may be productive of much evil; and that especially where stimulating vapours are employed, great care should be taken that little irritability of the pulmonary substance exists. A few words remain to be said as to the methods in which inhalation is usually practised. Different apparatus have been employed for the purpose, all being modifications of the same principle, and all demanding some effort and practice on the patient's part. It is argued that we can in this way better regulate the quantity of the vapour inhaled; and without doubt this is true, but the method is still not without considerable disadvantages. Many persons find great difficulty in acquiring what may be named, the art of inhaling with facility, through a contracted tube, or even through one whose area is considerable, and at best, the act is a constrained one and only accomplished by some exertion. These are evils which, in many examples, render it difficult to carry this plan of treatment into effect, and which not less seldom interfere with its efficacy, even where the patient willingly co-operates. There seems to be one way only in which this difficulty could be overcome—a method frequently proposed, but except in the case of tar vapour as suggested by Crichton, seldom if ever executed. I mean the introduction of the patient into an apartment charged with the vapour we desire to employ; by such an arrangement, the remedy would be applied during that equable, and unconscious expansion of the chest, which natural respiration involves, and the patient saved from the constrained effort which always accompanies an act of suction. There are indeed practical difficulties connected with such arrangements, not always readily overcome in private houses; but there seems no sufficient reason why an establishment for the purpose might not be formed, containing rooms whose atmosphere might be charged with different airs or vapours: to these, patients might occasionally resort for such a period as is deemed expedient; and it appears to me that by adopting such a plan can we alone arrive at a perfect acquaintance with the true amount of benefit which the local treatment of phthisis is calculated to confer.

SECTION IV.

Treatment of Symptoms:—1. *Inflammatory Complications.* 2. *Hectic Fever.* 3. *Hæmoptysis.* 4. *Diarrhœa.* 5. *Colliquative Perspiration.* 6. *Cough.*

1. INFLAMMATORY COMPLICATIONS.—It is unnecessary again to go over the arguments which, with the concurrence of Laennec and others, I have before employed to show, that tubercles of the lung are neither the result of inflammation nor always excite it after their deposition. But admitting this view to be correct, a very slight acquaintance with phthisis must serve to determine, that symptoms of a decidedly phlogistic character are often present during its progress, and frequently constitute the immediate danger; nor is it of much practical importance whether these depend on a secondary condition induced by the main disease, or are to be viewed as the manifestations of an associated affection not holding with it any direct connexion; for in each case we have to encounter a disease rapid in its progress, and which, if permitted to proceed, either destroys the patient speedily by

inducing the common effects of inflammation in the lung, or by hurrying on the tubercles into rapid and fatal softening. Under such circumstances the immediately important object of practice is that of removing the congestion or inflammation, and although the means necessary for this purpose are not such as phthisis most properly admits, we are often driven to them from necessity, and not from choice.

Of all these, blood-letting is without question the most powerful either for evil or good, and it is one which, for this reason, ought never to be adopted without due consideration, and modified to a certain extent, by a proper regard to the essential malady, of which the symptoms presented to us are only effects.

When the oppression of breathing is great, and pain or uneasiness of the chest generally diffused, when the animal temperature is higher than that of nature, and the pulse not merely rapid, but tense, and as the term goes, wiry, in its character, offering strong resistance to compression, there appears small reason to doubt the propriety of venesection, the amount of blood emitted being regulated by the strength of the patient, and the period of the disease at which the necessity occurs. It is luckily at an early date that this contingency commonly arises, when the powers of the body are more or less intact; or at all events it is then, that a form of disease more perhaps congestive than strictly inflammatory, occurs; this I believe to depend on the obstruction caused in many of the pulmonic capillaries, by the progressive deposition going on; but whether this conjecture be correct or not, it is then certainly that an active practice is of most utility, and may be adopted with most safety; but where we have reason to know that excavations exist, and where prevailing emaciation and debility announce, in terms only too obvious, that pulmonary reaction has made extensive inroads on the constitution, there clearly exists a very sufficient reason for using great precaution in the employment of the lancet; here, nothing short of a decided urgency can justify its use. When the inflammatory excitement is not great, and the pain of chest even if acute in character, limited to certain points alone, the local abstraction of blood frequently offers a valuable resource, attaining all the objects which venesection could effect, without exhausting power to the same extent; this is especially the case with regard to the limited attacks of pleurisy, which so frequently distress the consumptive patient; the application of a few leeches over the seat of pain, or the abstraction of a very moderate quantity of blood by cupping, will often effect a more useful purpose than a much larger amount drawn from the arm. But even topical bleeding should not be rashly employed; it differs from general, more in regard to the quantity withdrawn than in the principle of its action, and each tends, though in different degrees, to exhaust the pabulum of life, which in a disease like consumption it is always of importance to husband, so far as can safely be done. It is here that counter-irritation so well applies. I believe it to exert no power whatever over tubercular disease itself; that it neither conduces to the absorption of the matter, nor accelerates its evacuation, nor stays the progress of its deposition; but I believe it at the same time to present the best means which we possess, of meeting many of its complications, and this, without leading to that exhaustion which, always more or less succeeds to evacuations of blood any how effected. The modes in which counter-irritation may be practised are extremely numerous, but the principle on which they act is probably the same in all. The theory of their action I will not here attempt to discuss, but advert to their practical utility alone. There is however one general proposition in relation to their use, which it appears to me may be properly laid down; that they seldom if ever do good when they excite the actions of life in any great degree; and so, where either from constitutional irritability in the patient, or from his position being at the moment, such as renders his skin peculiarly susceptible, or his tendency to exaggerated action great, the evils they produce are often greater than those which they subdue. We here perceive a reason why they should not be employed, when much febrile excitement remains; and why in a few cases, luckily rare ones, they can scarcely ever be adopted with advantage: most practitioners have met with patients in whom a blister, or other active irritant, always produces the most severe distress. This however is the rare exception, not the rule ; and in the vast majority of examples, we possess few if any more practically useful means in the complications of consumptive disease than the class of remedies here referred to. Among the special means of effecting counter-irritation, the cantharides has long held a first rank, and not without reason. The common blistering ointment usually acts conveniently, though a somewhat clumsy application ; a modification of it free from oily ingredients, has lately been introduced by Messrs. Smith of Edinburgh, which offers a much more elegant and equally effectual remedy. A solution of the fly in acetic acid, now adopted by the London College, applied by means of lint, and retained in contact with the skin till its effects are produced, is often a more convenient formula than either of the others. The ointment of tartar emetic, which produces a pustular eruption, is with many a favourite application ; its severity, certainly greater than that of most others, seems the principal objection to its use. Mustard flower, under the form of a cataplasm, presents an exceedingly manageable mode of applying the principle ; few counter-irritants act more speedily, or produce less permanent irritation ; frequent repetitions, and never permitting its contact to be so long continued at one time as to produce vesication, appear the chief cautions required in its employment.

The internal treatment of inflammatory action, as superadded to a phthisical condition of the lungs, does not essentially vary from that of pneumonia occurring in a healthy subject. The diet of the patient should, for the time, be reduced below the standard already suggested as proper during the progress of the fundamental disease, and barley water, gruel, or thin arrowroot constitute his only nourishment. As regards medicines, I have always found it expedient to abandon the use of the alkaline treatment so long as the symptoms of excitement last, and have recourse to those remedies which tend to diminish action; moderate saline purgation is properly employed, even at the risk of aggravating the generally prevailing state of the abdominal mucous membrane, for we have here a new disease to treat, and are bound if possible to check its progress, even by the application of means we should not otherwise have deemed expedient. Weak solutions of the non-purgative neutral salts, such as the acetate or citrate of potash, combined either with ipecacuanha or tartar emetic, in doses sufficient to keep up a moderate but continued nausea, are highly proper; in short all those means which experience points out as best calculated to subdue inflammatory action in important organs, ought to be sedulously employed; always however holding in view, that they cannot safely be applied with such decision, or carried so far, as when the conditions they are intended to remove occur in a patient of sound and vigorous constitution.

2. Hectic Fever.—It is unnecessary to examine at any length the opinions which have been held as to the causes on which hectic depends. By many of the nosologists it has been considered as a peculiar form of remittent, which frequently originates altogether independent of any local affection. This was the conviction of Boerhaave, Linneus, Willan and many others. Cullen and Heberden again view it as always symptomatic, and this seems to be the conclusion at which physicians in general have arrived. But while a local origin is thus usually assigned to the affection, it is not so easy to determine what are the particular errors, either as regards their nature or extent, out of which it springs, for undoubtedly many and severe diseases of important organs are daily brought under our notice, which are not attended by any such constitutional concomitant. This fact has led the advocates of its local origin to speculate on the particular conditions required for its production, but their investigations have not led to very satisfactory results. By many it was concluded to depend on the absorption of pus, a belief clearly untenable, for while on the one hand we know that this fluid is often taken up in large quantities, without hectic supervening, we also know that the fever frequently occurs when there exists no evidence of suppuration in any organ of the body. Local irritation of some description however would appear a necessary precursor,

though it must be admitted that the seat of this is not at all times very obvious.

Without however entering further on the general question, it is sufficient for our present purpose to know, that few if any cases of phthisis occur, without being attended, at all events in their later stages, with that peculiar form of fever which falls under the definition of a true hectic; and indeed so generally is this the case, that with many writers, the fever attracts the larger share of attention, and their remedies are directed chiefly to its removal, as if the pulmonic affection was the effect, and not the cause of the constitutional symptoms which attend. Modern pathology has sufficiently dispelled this illusion, and we now know with certainty that the fever results, either from those primary functional aberrations which go to establish tubercles in the lungs, or from the morbid re-action of these organs after their occupation. It would on this view be absurd to suppose that any means can be effectual in the cure of hectic, which do not act on the main disease, and hence its treatment is essentially involved in that of tubercles generally, a subject before sufficiently discussed. But as a symptom may often be mitigated, though not removed, even during the continuance of the disease on which it depends, so we have here the power of often saving the patient much discomfort, even where our efforts are ineffectual in combating the parent malady. Hectic fever is extremely irregular in the accession of its paroxysms, and not less so in the relation which its different stages bear to each other ; noon and evening have been stated as the common periods of invasion, and this is certainly the general, though not the universal rule. The attack commences with a cold stage, not always very distinctly marked—like other fevers of its class, a hot one succeeds, which is always more appreciable, and ends with one of perspiration, which is the most distinct of all. This latter one is never absent, and as a general rule is altogether disproportionate in duration, and amount of excretion, to the others ; it is this indeed which leads to the greatest discomfort, tends to debilitate the powers of the patient most, and consequently forms the symptom towards which remedial attempts are principally directed. Cinchona, the great specific in intermittents, and which, on analogy, would appear so promising a remedy in the case before us, is seldom productive of benefit; the cause of the constitutional disturbance is too permanently and deeply rooted to be much influenced by its powers: advantage has been supposed often to follow the use of myrrh, and when combined with iron, in the formula originally employed by Dr. Griffith, and now found in our Pharmacopœia under the name of the mistura ferri comp., it certainly appears often to exert a salutary control. When the cold fit is severe, the application of external heat, and the internal use of warm diluents, are useful means. During the hot one, the patient should be lightly clad, and his apartment airy and well ventilated; sponging the surface with vinegar and water is here also a valuable practice; for by moderating cutaneous action at that time, we may hope to succeed in diminishing the subsequent exhausting stage of perspiration. Over this last and most harassing symptom the acids, mineral and vegetable, have been long presumed, and not without reason, to exert the greatest influence; and when the question has resolved itself into the treatment of symptoms only, all hope from a curative practice having been abandoned, they are remedies to which it is often expedient to have recourse.

Such appear to constitute the principal means[*] calculated to mitigate the symptoms of hectic, considered in relation to itself alone: means whose powers must ever be restricted, seeing that the fever is dependant on a local cause. In the case before us that cause is, without much question, tubercles in the lungs, and its only effectual and lasting remedy must be looked for in such practice as controls their deposition. To the means, which, in my belief, are best calculated to attain this end, a large portion of the preceding pages are devoted; and where these succeed, the symptoms decline or terminate with the removal of the cause which produced them. In any event, I may assert with tolerable certainty, that hectic fever will be rendered more mild and manageable, by a strict adherence to the regimen and mode of medical treatment throughout inculcated—that it will be aggravated by the employment of a highly nutritious and stimulating diet, and by those, so called tonic appliances, which on a most erroneous hypothesis are not unfrequently resorted to. I may end by using the words which head an early section of this chapter, that the restriction of systemic action, while it co-operates powerfully in the removal of the essential disease, exerts not less influence in moderating the fever which so generally accompanies its presence.

3. Hæmoptysis.—The time has long passed away, since men influenced by a blind adherence to the doctrines of Cullen, considered it as one of the first of medical truths, that phthisis was invariably a result of pulmonary hæmorrhage; and now, no proposition is more generally acknowledged, than that this symptom is not present in very many cases, which in their origin, progress, and termination, are distinctly marked by all the characters of genuine consumption. Such is the ever-varying, uncertain nature of all general principles in medicine. But the mere fact that a man of Cullen's acuteness of observation, should have selected this symptom as a universal concomitant, or rather as the cause of the disease, might alone be sufficient to show that it is one of very frequent occurrence, and in this, all modern observers are agreed; for, although bleeding from the lungs takes place frequently where tubercles are not present, it is still so commonly connected with

this form of deposit, as to lead to a strong suspicion of its existence, even where the fact is not by other signs distinctly marked.

Bleeding from the lungs, as dependant on tubercles, occurs under two forms, and owns two distinct causes. The first, and by far the most usual variety, presents itself early in the disease, and would seem seldom if ever to be connected with any actual rupture of vessels; but the blood oozes into the bronchial tubes, or air cells, from the mucous surface, congested or inflamed as a consequence probably of obstructions caused by existing tubercles. Later in the disease again, when excavations more or less extensive are present, the bleeding often appears to depend on a breach of continuity in one or more pulmonary vessels of some size; and indeed, when we observe how arteries of considerable magnitude run along the sides of these excavations, with little support beyond what their own coats supply, we are led to wonder how such an event does not more frequently occur. Of these two kinds of hæmoptysis, the first is however the more frequent, and forms one of the most alarming and important symptoms which phthisis presents; for when it proceeds to a great extent, not only may it induce irremediable debility, from the quantity of blood emitted, but when this is only small in amount, tend greatly to accelerate the progress of the original disease, by consolidating, and abstracting from respiration, considerable portions of the lung.

Pulmonary hemorrhage of this sort is invariably accompanied by increased excitement of the vascular system, and distinctly comes under the definition of an acute symptom, hence demanding the application of those means which exert a control over the motions of the heart and vessels. Of all such, blood-letting is the most powerful, and however valid the objections to this remedy may be, in so far as relates to tubercular disease itself, there can be little doubt of its propriety when phthisis is attended by this, its most serious complication. The quantity of blood proper to be taken must here, as in other cases, depend on the general combination of symptoms present—the strength and robust condition of the individual—and the period of his disease; but one general rule may safely be laid down, that when the hemorrhage is severe, and the symptoms of fever height, venesection should be carried to such an extent as makes a decided impression on the pulse; for so long as this remains hard, wiry, and jerking, we can feel no confidence that the symptom will not recur, even should it be for the time suspended. It often happens that the pulse becomes quieter, and the pulmonic blood ceases to flow, when only a limited quantity has been abstracted by art; in such an event it is generally wise to stay the operation, even at the risk of being again called on to perform it, for the mere suspension of the hemorrhage affords us time to adopt the use of other measures of less severity, and in their effects less debilitating, which, if they succeed, may spare us the necessity of further recurring to a means of treatment to which we are often driven merely on the principle of selecting the lesser of two difficulties. The internal remedies most influential in combating hæmoptysis, are those which directly or indirectly moderate or control systemic action. Whether bleeding has been practised or not, a smart purgative, of the saline order, should be at once given, and repeated as circumstances may demand; for by stimulating the bowels to serous discharges, we diminish not only the general bulk of blood in the body, but aid in the removal of that congested state of the liver and abdominal viscera, which, as Sir James Clark has most properly stated, is very commonly present. With the same intention, a moderate dose of calomel, combined with a grain or two grains of ipecacuanha, should precede the purgative draught by a few hours. Of the specific sedatives which have been supposed to control hæmoptysis, the acetate of lead is perhaps the most powerful, and when combined with opium, may be more freely and safely given than many have supposed. In those cases more particularly in which we have been compelled to push blood-letting to some extent, I have myself found no treatment so effectual, as that of prescribing from two to four grains of the acetate, in combination with from a quarter to one grain of opium, every three or four hours. Where depletion has been carried far, there occurs very commonly a vascular re-action, which, as long ago remarked by the late Dr. Armstrong, is more effectually controlled by opium than by any other remedy: and it seems to me, on this principle, that it here decidedly augments the sedative powers of the lead. Independent of this advantage also, the irritative action of this metal on the bowels is less likely to be shown when it is so associated.

Under the title of indirect sedatives, may be classed all those medicines which tend to lower action by the primary induction of nausea; of these ipecacuanha and emetic tartar are the two in common use, and each of these remedies has its exclusive advocates; to myself it appears really of little consequence which is employed, provided the object of producing moderate sickness be gained. There is however one exception to this rule, which is often of importance; when the abdominal surfaces are in an irritable state, the ipecacuanha is the more safe of the two. It is always a good practice to combine a portion of one or other of these remedies with the purgative draughts employed, and so long as the bleeding either continues, or the condition of the circulation renders its recurrence probable, to moderate vascular action by small doses frequently repeated.

When we have reason to think that the hemorrhage has been connected with inflammatory excitement of the lungs, and when, after the more active measures already described shall have failed entirely to remove it, the use of blisters, or other counter-irritants adapted to the case, should never be neglected. On the precautions necessary to be observed in their adoption I have before sufficiently remarked.

The evils of pulmonary hemorrhage do not terminate with its cessation. Independently of the blood coughed up, a certain portion always finds its way into the minute bronchi and cells, and either permanently, or under more favourable circumstances, for a time, consolidates certain portions of the tissue, and abstracts a certain amount of lung from respiration, thus aggravating all the numerous dangers of tubercular disease. When the case proceeds favourably, this blood is ultimately absorbed, as shown distinctly by the frequent return of the respiratory sound in parts where it was before inaudible. In others, the consolidation is never removed. Over this secondary difficulty which hemoptysis engenders, I believe that we possess little control, except in so far as, by adopting a proper mode of general treatment, we can improve the condition of the patient's functions, and along with others, invigorate the powers of his absorbent system. Such however is by no means the opinion of many, who rely with much confidence on emetics, for mechanically dislodging and evacuating the effused blood.

So far as this is contained within bronchial divisions of some size, I can readily admit the probability of such effect being produced, and there are certainly many cases in which obstructions so situated, and leading to severe and even dangerous dyspnœa, are best relieved by the full action of an emetic; but where the blood has found its way into the true breathing tissues of the lung, I greatly question the efficiency of such a treatment. So long as active hemoptysis continues, or a tendency to it exists, whatever medical means are used must be aided by a strict attention to the diet and general management of the patient; the blandest food and mildest cold drinks should alone be permitted him, the room in which he lies kept well ventilated, and at a moderate temperature, and his bed-clothes or other coverings be of a light kind; all muscular effort, all excitement of mind, or exertion of the voice in speaking or reading, should be carefully avoided,—in short, every cause which conduces to the extension of vital action, should be as far as possible abstracted.

When hemorrhage from the lung occurs profusely and suddenly, late in the disease, it may with much certainty be set down as proceeding from ruptured vessels of some size. Here the patient sometimes sinks at once from the mere effects of the effusion, but when time for practice is permitted, its nature must be varied according to the general aspect of the case. If, as at times happens, excitement of the circulation attends, it must be met by measures not dissimilar in kind to those already mentioned; modified however in extent, by our knowledge of the existing debility of the patient. When again hemorrhage of this kind occurs in a highly atonic state of the system, astringents and corroborating remedies hold out our only, though unfortunately most uncertain resource.

4. Diarrhœa.—The same constitutional diathesis which leads to the deposit of tubercles in the lung, also very frequently leads to their presence, at the same time, in other organs, and of these none are so frequently affected as the bowels. That this is the case morbid anatomy amply demonstrates. Of a hundred and twenty-three bodies examined by Loui, one-third presented tubercles in the small, and one-ninth in the large intestines, and Lombard found the bowels attached in twenty-six cases out of a hundred.*

The immediate seat of intestinal tubercles appears to be the cellular membrane, which connects the villous and muscular coats, and it would seem to occupy the mucous follicles which naturally exist in that situation. At first they form slight elevations of the mucous membrane only; by degrees the tubercular matter, following its usual course, softens, the membrane becomes irritated, ulcerative absorption ensues, and abrasions, sometimes extensive, are produced ; occasionally those ulcers are so deep that the whole inner and middle coats of the bowel are destroyed, and its serous covering alone prevents the escape of its contents. In some more rare examples even this gives way, and the patient dies suddenly from effusion into the peritoneal cavity. It is obviously on the condition now described, that the worst and most intractable form of diarrhœa in phthisis depends, presenting a symptom extremely harassing to the patient, and which tends rapidly to exhaust the little strength which remains to him. Over a complication of this kind, dependant generally at least on extensive structural changes, it is not to be hoped that the art of medicine can ever exert a curative influence, and all we can look for is, to aid in the mitigation of suffering which cannot be entirely removed. More of our success in this depends on general management, than on the administration of any special remedies. The diarrhœa is certainly aggravated by a stimulating and highly animalized diet, and as surely mitigated by mild, unirritating, and chiefly farinacious aliment, so that the same description of nutriment which has before been suggested in the general treatment of phthisis, is also that most likely to avert or alleviate one of its most severe and hazardous symptoms.

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* In Loui's one hundred and twenty-three cases, tubercles were found in the small intestines in 1-3rd; in the large intestines in 1-9th; in the cervical glands in 1-10th; the lumbar glands contained them in 1-12th; the prostate in 1-13th; the spleen 1-14th; the ovaries in 1-20th; the kidneys in 1-40th. Lombard, in a hundred cases of phthisis, found intestinal tubercles in twenty-six; nineteen cases presented them in the mysenteric glands; nine in the bronchial, seven in the cervical, and three in the maxillary. The cellular membrane beneath the pectoral muscle contained them in four examples, and the mediastinum in four. The arachnoid membrane, the spinal marrow, the serous membranes, the intercostal muscles and the ovaries, were each tuberculated in two cases only.

Opiates and other anodynes are the medicines from which in my own experience the greatest advantage is derived; from chalk mixture, a remedy, which it is a sort of routine practice to prescribe in all forms of diarrhœa, I have never seen much benefit, it forms however an innocuous vehicle for anodynes, and as such is convenient; astringents, especially catechu, sometimes appear to be slightly beneficial, but their influence is seldom permanent, and I may again repeat that, in my own conviction, opium and narcotics generally constitute by far our most valuable resource. When diarrhœa occurs comparatively early in phthisis, it is not always connected with tubercular degeneration of the bowels, but would appear frequently to depend on irritation or congestion of the mucous coat. There is much reason to think that these conditions are often connected with obstructions in the liver, and mild mercurials are hence indicated. From moderate doses of the hydr. c. creta combined with Dover's powder, or from small quantities of blue pill associated with opium or other anodynes, great benefit is frequently derived, especially when followed up by a mild laxative.

5. Colliquitive Perspiration.—On the treatment of hectic generally I have before commented, and little further remains to be said of the means best calculated to alleviate perspiration, which must be in a great measure viewed as the third or final stage of that peculiar form of remittent. As then stated, whatever tends to diminish the fever, tends

also to limit the perspiration, and whatever augments the one, augments likewise the other. Some however appear to treat of this as an isolated symptom, demanding special measures applicable to itself. I believe this to be an erroneous conclusion, founded merely on the circumstance, that the sweating stage is often more marked and longer continued than the cold and hot ones which precede it. This however is in a great degree a speculative question, in itself of little moment, but sure I am that we possess no such certain means of checking the copious drain proceeding on the skin, as by the adoption of that general management both as regards diet and medicine, which has before been repeatedly urged on the attention of the reader. Considering perspiration simply as a detached symptom, it occurs to a greater extent at night, and it is hence of much importance that the linen should be very frequently changed; thick soft calico is the best fabric which can be applied next the skin, it absorbs moisture readily, and possesses over even thin flannel this advantage, that it less retains the animal heat, an occurrence which, during the prevalence of the perspirations, it is always of importance to avoid. For a similar reason the bed-clothes of the patient should be as slight as he can submit to without inconvenient chilliness resulting, and the skin should be frequently dried with a soft cloth; this is no unimportant point, for by permitting a continued evaporation from the surface, a sensation of cold is often produced which impels the patient to call for external warmth, artificially produced, often by means which are improper. Sponging the surface with vinegar and water is always comforting, and often obviously useful. On internal treatment I can offer nothing more than has been already said.

6. Cough.—When we rightly consider the nature of that involuntary, convulsive effort of the respiratory muscles, to which the term of cough is applied, we arrive at the conclusion, that it depends on an effort of the system to relieve the lungs from some irritation which besets them, and impedes their function. At times the effort is effectual, and the relief permanent, because the interrupting cause is removed, and not necessarily reproduced; this happens when a foreign body finds entrance to the trachea, or lodges near the sensitive orifice of the larynx. In a second form, the effort is excited without its being capable of removing the cause; this takes place in the earlier stages of consumption, when tubercles are yet unsoftened, and in catarrh, bronchitis, or pneumonia, before the vessels have begun to relieve themselves by secretion. In a third variety of cases the act of coughing for the time meets the evil, it throws forth the offending cause, but becomes an established symptom of the disease so long as this cause continues to be reproduced.

The cough which attends on phthisis, in its different stages, is referrible to two of these varieties. Early in the disease it is dry, short, and hacking, clearly dependant on irritation produced by the forming tubercles, and effects no purpose beyond that of freeing the air tubes from a slight increase of secretion; the expectorated matter is therefore more or less transparent, and secreted by the mucous surfaces alone; by degrees its character changes, and detached portions of softened tubercle become mingled with the bronchial secretions; this augments as the malady proceeds, and towards its termination the expectoration appears as if composed chiefly of the adventitious matter alone. We thus perceive that cough is a symptom necessarily connected with consumption, that its cause is constantly recurring during the entire progress of the complaint, and that the idea of curing it is absolutely chimerical, except by remedying the conditions it results from. Nor, did we even possess the power of so doing, would the proceeding be either a safe or proper one. There seems every reason to suppose that tubercles when once deposited, are rarely if ever absorbed again; the only mode in which the matter can be removed is therefore by expulsion from the trachea in a softened state, and this cannot be accomplished without cough. It is right that this matter should be properly understood by patients, for they are extremely apt to look on the symptom, as the disease, whereas the continuance of the symptom is always necessary towards a cure, when this happily occurs. Having offered those hints on cough generally, it remains to say a few words on the powers of alleviation which medicine can exert over it, as it occurs in phthisis. Early in the disease, as already stated, it is of an irritative kind, and unattended by much expectoration. Here the true object of practice is to check the progressive deposition, a point already sufficiently discussed, and the probable means of its accomplishment strenuously urged. I shall here only add that in this stage I have found no remedy so effectual in combating the cough as alkalis, to which for the purpose of gaining a temporary advantage, small doses of a mild narcotic should be added, and, when it is harsh and dry, a portion of the solution of ipecacuanha, in spirit—not wine. At times under this management, the cough either ceases, or much declines, because, as I conceive, the progress of deposition is arrested, and the tubercles already formed often remain in a latent state, and produce, with caution on the patient's part, little inconvenience. This is however the most favourable result, and certainly also the most rare one; more commonly, the existing deposit tends to soften, and a cough, attended by tubercular excretion, becomes established. Under all these circumstances the alkaline treatment should be steadily pursued as the basis of our practice, but the ordeal through which the patient must pass, whatever be his ultimate fate, may to a certain extent be rendered less severe. Here, of all remedies, narcotics are the most valuable, and among them none answers the purpose so well as the pure extract of lettuce. Of the preparations of opium, the acetate of morphia, or the analogous preparation, Battley's solution, have appeared to answer best. The extract and tincture of conium, are valuable preparations, and the same may be said of the hyoscyamus. One and all of these remedies are rendered more safe and effectual by their union with moderate doses of the ipecacuan.

The best vehicle in which narcotics can be given, are the various mucilagenous mixtures so generally employed ; they at least tend to amuse the patient, and at times appear really useful, by quieting the uneasy irritation which exists about the orifice of the larynx. When the cough seems aggravated by the presence of inflammatory excitement, the same principles of treatment before alluded to of course ought to be applied.

CHAPTER VI.

REPORT OF CASES IN WHICH THE PRINCIPLES OF TREATMENT INCULCATED WERE PRACTICALLY APPLIED; WITH SOME REMARKS ON THE DIAGNOSIS OF PHTHISIS, MORE ESPECIALLY IN ITS EARLY STAGES.

Having now brought to a conclusion the observations I have thought it necessary to make, both in reference to the pathology and general treatment of consumption, it remains to conclude this volume with a report of some cases, selected from among many more of a parallel description, which would appear to confirm in some measure the correctness of the conclusions contained in it; and which seem more especially to point out the value of that alkaline treatment, which it has been my leading purpose to bring under the notice of the profession.

To render reported cases of much or indeed of any value, two things are required—honesty on the part of the reporter; and, should this be admitted, sufficient evidence that he has not been self-deceived, but really treated the disease which he professes to have benefited or cured; and this is especially necessary in regard to phthisis—a malady whose early diagnosis is well known to be attended with such extreme difficulty. The first of these two requisites it is not in the power of an author to supply; his own assertion of honesty of purpose, in reporting facts, obviously goes for little, and he can therefore only depend for credence on the general confidence which he may have had the good fortune to establish in the minds of those who know him. This part of the subject I shall therefore dismiss with one remark; that I have endeavoured, so far as lay in my power, to communicate results faithfully; and in the practical application of the principles I advocate, to disabuse my mind as much as possible from any bias it might naturally tend to receive from the influence of a favourite theory.

The second requisite I have alluded to, involving the nature of the diagnostic evidence relied on, for proving the existence of phthisis in the cases noted, may be supplied with greater confidence. On this subject I shall now briefly proceed to offer a few general comments, leaving it for others to determine whether it has been such, in each special case, as to warrant the supposition that pulmonary tubercles had a positive existence, where success attended the application of the measures used.

Percussion and auscultation are without doubt the best and most certain means of diagnosis, which phthisis admits; and were we limited to one set of marks alone, we should scarcely hesitate to adopt them in preference to all others. They are not however infallible—and I hold that in the very early stages of the disease, we are scarcely justified in concluding from them only, that tubercles are present, unless the evidence they offer is also corroborated by the history of the case—the hereditary tendencies of the patient, and the nature of the symptoms which are present.

The principal signs on which auscultators have relied, in concluding the existence of tubercles at an early date, are of three kinds ; greater or less dullness of sound on percussing the upper regions of the chest ; absence or diminution of the respiratory murmur in the same situation ; and resonance of the voice or cough. When these signs are all very distinctly present, they announce with something approaching to certainty an existing consolidation of the lung ; and, when they are altogether or principally manifested in the upper lobes, they go very far to determine that such consolidation depends on tubercles ; for it is fully admitted, that it is in such situations, that these bodies almost invariably first show themselves. The evidence is much strengthened if the lower lobes remain respirable ; and all but certain if these signs are unequivocally marked in a patient, whose hereditary tendencies, general aspect, and prevailing symptoms go towards their confirmation. It is not, however, always that the signs themselves are very definitely marked, or that they are satisfactorily confirmed by the general symptoms ; and then the evidence is more or less conclusive in the exact ratio, of the presence or absence of one or more, or of the intensity or feebleness in which they exist. Having offered this general remark, I shall briefly present a very few more on the relative value of these different indications.

In the first place, I conceive that many cases even of incipient phthisis come before us, in which we are able to decide with great confidence, altogether apart from any reference to physical methods of diagnosis. The general aspect of the patient—almost intuitively recognized by those accustomed to observe it; the panting respiration—the quick irritable pulse, inordinately accelerated on the least exertion—the usual presence of struma in some external organ—and the contracted, flat contour of the chest, present a combination of symptoms which it is all but impossible to mistake; and we shall seldom find that these are not amply confirmed, by a recourse to auscultation and percussion. It is not however always that symptoms are so distinct, and without question, in numerous examples the general signs amount to suspicion only, while the physical ones, either render these more conclusive, or at times announce tubercular occupation with precision, where the others are too indistinct, to attract much attention or lead to much alarm. It is therefore especially proper that we should duly consider their relative value, and estimate properly the circumstances under which they may without deception be relied on.

The information furnished by percussion, as indicative of pulmonary tubercles in their very early stage, I believe to be more valuable than that which auscultation conveys. When these bodies are not numerous, and placed at some distance from each other, the intervening portions of lung often continue so fully permeable by air, that the respiratory murmur is heard with scarcely less distinctness than in health, or at all events the shade of diminution is too slight to be recognized, except by a very acute and practised ear. It is indeed true that its character is frequently altered, and that it becomes more harsh, grating, and bronchial than before, but as this, or something very similar, takes place in many acute affections of the organ before secretion commences, the mere presence of such sounds cannot be well relied on, unless confirmed by collateral evidence.

Another objection to depending on the respiratory sound alone, appears to be, that the indications derived from it are more of a negative than positive kind. In many persons of weak power, but possessing healthy lungs, the murmur is, naturally, so feeble, that it can scarce be heard, and were we in such examples to be guided by its presumed absence, the fallacy involved becomes at once apparent. We may conclude therefore, that when vesicular breathing is clearly and distinctly audible, we possess tolerably strong, though by no means certain proof, that the lung is either not occupied at all or only to a small extent; but, that the mere absence of this sound is not to be taken as direct evidence of solidity, and can only be looked on as a circumstance of some value, in enabling us to arrive at a correct opinion. Percussion again seems here to afford more decisive data. In healthy chests, or at least in those of persons who have not presented any symptoms of pulmonic disease, we not unfrequently find that it elicits a sound in every way normal, when auscultation only reveals a very indistinct murmur. The converse of this is often true in incipient phthisis, the sounds resulting from percussion being frequently dull, while vesicular respiration is still distinct. This happens in a large number of cases similar to those before referred to, whose breathing being naturally loud, have unsoftened tubercles disseminated through the lobes with respirable portions between them, and would appear to depend on the circumstance, that it is more easy to discriminate minute differences, in the sounds procured by striking the chest, than in those transmitted to the ear from its interior. We are luckily however, not obliged to depend on either of those methods of diagnosis singly taken, and should always bring both to bear on the same case. When respiration of a healthy character is heard, and when in the same situations, percussion educes a natural sound, we may conclude that the lung has either not suffered at all, or at least not extensively. When again decided dullness of sound follows it, and when this concurs either with the absence of the respiratory murmur, or with great diminution of it, a reverse and therefore unfavourable deduction may with great safety be formed.

Auscultation of the voice, or cough, provided one or both are strong enough to admit its application, presents a valuable means of investigation, for experience seems to determine that the lung cannot be consolidated to any great extent without their sounds being conveyed through the stethoscope to the ear. There is one source of fallacy, however, connected with this method, which should be carefully guarded against. It is well known that when one end of the instrument is placed over the course of the trachea in a healthy person, a distinct resonance of the voice is transmitted through it to the other; the same phenomenon occurs, when it is placed over a large bronchial division, and hence, when we apply this method to the superior regions of the chest, great care must be taken to avoid those situations where the trachea sends off its large divisions to either lung. Holding this in view, we ought little to rely on it, unless the resonance is heard, at or under, the acromial end of the clavicle, or high up in the axilla, or behind, as far as possible from the spine. If, using these precautions, the resonance is distinctly marked, it presents us with a point of evidence strongly corroborative of those which we may have otherwise acquired. The positive evidence of this sign is more valuable than the negative. It demands the existence of a certain strength and clearness of voice, often wanting; and unless this exists, the absence of resonance must not be permitted to weigh much against proofs derived from other sources.

Independent of these principal points in the physical diagnosis of early tubercle, there are three others of minor importance, because not so generally present, and liable to increase or diminution from circumstances which we cannot always well control. The first of these is the flatness and diminished expansibility of the chest. When an act of inspiration is accomplished, in a healthy state of the lungs, the ribs tend upwards and outwards in a free and easy sweep, each rib seeming to follow the other in succession; but when extensive consolidation of the lung takes place, the respiratory muscles drag them, upwards only and as it were in a mass,—a motion not very easily described, but seldom forgot if once observed. This is more obvious in proportion as the consolidation is great, and the inspiration strong; and most so of all where one side of the chest is much healthier than the other, for then, the relative flatness of the one is rendered obvious by contrast, even during its quiescence. It may form a question whether the apparent bulging and increased rotundity of the healthy side is merely the result of contrast, or whether, the one enlarges as the other contracts. I believe this last to be the case, and that it depends on the healthy lung being called on to perform a double action, in consequence of the inactivity of its fellow; at all events, under such conditions the sound elicited by percussion, while dead on the one side, is often abnormally clear on the other, and the respiration in it carried on with greatly exaggerated force.

A third and last minor point of evidence is only available when the right side is affected. In health, the sounds, and impulse of the heart, are usually perceptible under the clavicle of the left, but not of the right side, unless it be either pulsating with great force, or that some consolidating cause acts on the right lung; when therefore, the heart's action being moderate, we find the sounds and impulse of the organ distinctly transmitted to the outer end of the clavicle on that side, we may fairly receive it as an additional proof of existing disease, and the accuracy of the diagnosis will seldom fail when the sign is well marked.

But we must not forget that all these means of diagnosis, however properly applied and combined, conduct us after all only to one conclusion; they announce that some portions of the lung are consolidated; but they leave the cause of this almost entirely unexplained; and therefore, admitting their value to its full extent, we are bound to take also into account, the general circumstances and symptoms which are present in each case, before we can rely on them with any certainty; without so doing they may indeed conduct us to a conclusion, probable, but not positive.

To attain distinct evidence of the existence of tubercles while yet recent, is of more importance than afterwards, on two accounts. In the first place, it is much more difficult of acquisition; and in the second, is of greater value as regards treatment; for in the mass of cases, success must ever depend on an early adoption of remedial means. A few words only must therefore suffice on the additional means of diagnosis applicable to advanced phthisis. Many of those already mentioned, here also come to our aid, for the larger portions of the diseased parts still continue solidified when excavations are present. Independent of this, the general symptoms are better defined. The emaciation, the copious tubercular expectoration, the night perspirations, and often the diarrhœa, form a mass of evidence impossible to resist. I need scarcely add, that when all these are confirmed by the presence of pectoriloquy, no doubt can any longer remain.

In the reported cases which conclude this chapter, it will be perceived that I have in each inferred the presence of tubercle from a combination of the signs and symptoms already noticed. These cases I have arranged under three heads. Under the first are placed, examples of phthisis in its early stage, as appeared to be made out in a manner tolerably satisfactory. They are casually taken from about 250 of a parallel description, which during the last ten years I have treated essentially with the alkali; and although I am by no means prepared to say, that in all the symptoms may not have recurred, or the disease gone on to even a fatal termination; I may affirm, with great confidence, that for the time, all derived decided benefit from its use.

Under the second head I have reported three cases which differ from the others, only in appearing to offer some evidence of the absorption of tubercles in their unsoftened state. Of the fact, that consolidation of a portion of the lungs was removed in these examples, I cannot entertain a doubt, but am well aware that the proof of this having originally depended on tubercle, may to some appear imperfect. The subject, however, is one well worth further investigation.

In the third and last class, are placed a few cases, in which evidence of phthisis in its second stage being perfect, the symptoms declined or disappeared under the use of the alkali. These formed the rare exceptions to about 150 which were treated with it in this stage. In the vast majority, the disease ran its usual course, little influenced by the remedy, but even these few appear to me to show that we are not warranted in abandoning hope under most unpromising circumstances.

The cases, amounting to above 400, from which those reported have been taken, were chiefly treated by me in Dispensary practice; first at the Finsbury Dispensary, and afterwards at the Institution I am now connected with. I need not therefore add that the practice has been tested under the most unfavourable circumstances. Neither is it possible for me to offer a satisfactory or correct account of permanent results in the majority of examples; the migratory habits of the patients would render the attempt impracticable. Those who apply early in the disease, having obtained such relief as treatment can bestow, it is usually difficult and often impossible afterwards to trace; and those who present themselves in an advanced stage, finding little immediate benefit from medicine, often decline it altogether or resort to some other practitioner; of the ultimate fate of the majority of these however little doubt can be entertained.

In selecting the cases, I have endeavoured to render those under the first head—by far the most important—fair average examples of the larger number they are drawn from, which all presented evidence more or less distinct of tubercle actually located in the chest. I may here add, that the same plan of treatment has appeared to be equally or more successful in that large class of young persons, who often present all the symptoms of a strumous or phthisical diathesis, without the lungs having as yet apparently suffered.

CASES

In which the Treatment appears to have conduced to the quiescence of existing Pulmonary Tubercles, and arrested their tendency to increase.

I. Mary Pryor, aged 33, married; a tall thin woman, with narrow contracted chest and prominent shoulders, became my patient at the St. Marylebone Dispensary on 4th August, 1839. She reports herself as having been always peculiarly liable to "take cold," and on many occasions has spit blood; the symptoms she immediately complains of, commenced about a month ago. Her cough is constant and severe, attended with the expectoration of a creamy phlegm,—the pulse is 90, and she has no pain in the chest. Catamenia regular until within two months back; since which period they have appeared more frequently than usual—the bowels are rather relaxed, and dyspnœa succeeds to the slightest exertion.

Pryor is the only child of a family, but her mother and two aunts by the same side, died of consumption.

The physical signs in this case were less distinct than might have been anticipated from the general history and symptoms.

The upper regions, both anterior and posterior, sounded dull on percussion; on the right side, the respiratory murmur was diminished, and bronchophony, clearly, but slightly marked; the left side of the chest did not present any marked signs, and I could discover no evidence of a cavity to account for the expectorated matter, which in itself presented all the characters usually met with in the pus of softened tubercles.

I prescribed as follows:—

℞ Mist. camph ℥vß.
Tinct. Hyosciami ʒi.
Liq. Potassæ ʒiii.

An ounce to be taken every four hours; also, four grains of the extract of conium with one of ipecacuan every night at bed time.

On the 17th of the same month, she reported her cough and dyspnœa to be much better; she has had no return of hæmoptosis, and states that "she had not felt so well for many months."

From this period her improvement was progressive. She continued the same medicines until the 2nd October, on which day she was discharged, being at the time free from any pectoral symptom; the expectoration had ceased, and she had considerably improved in flesh and general strength.

I saw this woman again on the 3rd of last May, when she applied on account of some trivial dyspeptic complaint, from which she soon recovered. Of her former malady she had no recurrence, but had passed through two winters without a single symptom referrible to the chest.

II. Laura Spokes, Rathbone place, aged 21, unmarried; a tall thin young woman, presenting many of the external characters of a strumous constitution, applied in January 1839.

Her symptoms were then as follows:—a cough, to which she has always been liable on slight exposure, suffered an aggravation early in the winter, and is now harsh and severe, especially at night. She expectorates a frothy mucous in moderate quantity, but has never observed it to be tinged with blood. The catamenia are regular as regards time, but scanty in quantity; the bowels usually confined. Her pulse when at rest numbers about 95, but is readily quickened to above 100 on the slightest muscular exertion. She is employed as a sorter at a large manufactory of playing cards, which requires her to stand much, and the legs and feet are apparently from this cause, affected by erythematous redness.

Examination of the chest conveys the following information. Percussion practised over the clavicular, mammary, and scapular regions of the left side, elicits a sound extremely dull; the respiratory murmur is in some points inaudible, and throughout the whole upper bones, very indistinct; under the distal end of the clavicle a strong resonance of the voice is heard.

On the right side of the chest similar signs are present, but in diminished intensity.

She was ordered the following formula :—

℞ Liq. Potassæ, ℥vii.
Tinct. Hyosciami, ʒi.

Twenty-five drops to be taken four times a day in a glass of water; from four to eight grains of the aloes and myrrh pill as occasion may demand. Her diet to be confined to white fish, farinaceous food, eggs and milk.

The patient continued this plan, with gradual but steady improvement, till the end of March, 1840, when she was discharged at her own request. Her state was then as follows:— Breathing easy, pulse numbers 80 in the minute, the bowels incline still to costiveness; no cough or uneasy feeling in the chest. The respiratory murmur appears more distinct on the left side ; the sound on percussion is more clear and the resonance of voice less distinct. In the right lung no difference is appreciable.

I had not an opportunity of seeing this patient again till she applied on account of head-aches, on 6th last October. She is, and has been, free from cough and other pectoral symptoms, since our last meeting, and her robustness has distinctly improved.

III. Mary Lucas, aged 25, residing in High street, St. Marylebone, applied to me in October 1839. She is a pale thin young woman, with light hair and eyes, who describes herself as having been a delicate child, and liable to frequent coughs on slight exposure, since she can remember. Her breathing is always short and easily hurried. I could not find from her that she had ever spit blood. About two years ago she was in the service of an eminent army physician, which she left in consequence of a severe attack of cough, attended by copious creamy expectoration. At this period she was confined for six months, and her master and another medical man pronounced her to be in an advanced stage of consumption. From this attack she rallied partially, but always retained her cough, which about a week ago became worse, and induced her to apply to me. The bowels act regularly, the catamenia are scanty but natural, the tongue clean, but its papillæ prominent; the pulse ranges about 100.

The left cavity of the chest, when percussed over its upper regions, emits an extremely dull sound, both before and behind, the respiration is very indistinctly heard, except over the larger bronchi, where it is harsh and sibillant. Resonance of the voice is distinctly marked. The signs on the right side differ little from the last in kind, but are less decided. She was ordered a mixture, consisting of three drachms of the liquor potassæ, five and a half ounces of infusion of cascarilla, and one drachm of tincture of hyoscyamus. To use the conium and ipecacuan pill at night when the cough was troublesome, to attend strictly to her diet, and avoid as much as she could, all violent exertion. The patient continued the alkali, sometimes with the bitter infusion, sometimes in other formulæ, till the 8th of April, 1840; at which time her health being perfectly re-established, she declined further treatment. The sounds of the chest remained unaltered, except that the bronchial respiration had ceased to be harsh. This young woman is since married, and has one child. When I saw her some weeks ago, she stated herself to be free from pectoral symptoms, and, that, with the exception of a severe swelling of the face during last spring, she has had no important illness.

IV. Robert Saunders, 2, David street, aged about forty, began treatment on the 15th of last February. He presented all the symptoms of incipient phthisis, which the physical signs corroborated. The upper lobes of both lungs, especially the right, were extensively solidified, his cough hacking and dry, and his liver and digestion out of order. His pulse numbered 100, and the least muscular effort inordinately hurried his respiration. Mild mercurials, gentle laxatives, and an occasional anodyne, combined with attention to his diet, in a week or ten days, benefited the abdominal symptoms. He was then placed on a mixture of seven drachms of the alkali to one of the tincture of hyoscyamus, of which he took in water half a drachm every four hours. On the 14th of April he was dismissed well. I mention this case to show how speedily the act of deposition will sometimes be arrested in its course by the use of the alkali; every appearance at least indicated that a fresh evolution of tubercles was here about to succeed those, of whose existence in the lungs evidence seemed to be afforded.

V. Edward Whyburgh, aged thirty-six, 13, David street, was first seen by me in December, 1838. He stated himself to have been for many years liable to cough, dyspnœa on slight exercise, and expectoration of a white creamy matter, occasionally tinged with blood. His tongue was furred, the bowels rather confined, and his pulse numbered 95. On the upper regions of the right side, the sound on percussion was dull, resonance of the voice distinct, and respiration audible, but feeble. On the left, percussion educed an extremely dull sound both before and behind down to the seventh rib. Behind, loud bronchial respiration was heard; in a few places vesicular breathing appeared to exist, but obscured by the predominance of the other. He was ordered half a drachm of the liq. potassæ every four hours in water, and the compound rhubarb pill occasionally. He continued the medicines with progressive benefit till the 3rd of the succeeding June, when he was discharged, free from cough and all pectoral symptoms. This man again became my patient in the following December, on account of a recurrence of the same symptoms. His chest presented then similar signs, and he was treated on the same principles, with very speedy and effectual relief. Two months ago he remained well, and has had in the interval no pulmonary complaints of importance.

VI. Henry Cattermore, aged 25, a livery servant, 3, York street, applied in May, 1839; he had long been subject to cough and expectoration of a white, muco-purulent character, often tinged with blood. He professes himself to be descended from consumptive parents, and in his own person presents the marks of a phthisical habit. Dullness on percussion of the upper regions of the chest—resonance of voice and feeble or absent vesicular breathing, appeared sufficiently to announce the condition of the lungs. These signs were especially marked on the right side. He was placed on the alkali, of which he took two drachms in the day, with an occasional anodyne or mild laxative, as the symptoms varied. He was also repeatedly blistered. On the first of July he abandoned treatment, being very nearly free from cough, and much increased in flesh.

VII. Sarah Baillie, a young woman, aged 18, was admitted a patient, at the Finsbury Dispensary, on the 3d March, 1834; she was described as a girl always of delicate constitution—irregular in her catamenia—liable to take cold on the most trivial exposure, and suffered from dyspnœa on the least exertion; she besides presented, all the characters of the phthisical diathesis, and had a strong hereditary tendency to the disease. Her father is one of a family of eleven, five of whom died consumptive; and her mother the only survivor of seven, six of whom were carried off by the same complaint. Her cough, which had now existed for three months, was of a dry hacking character, with slight frothy expectoration, very generally tinged with blood; she was now very slight and thin, a condition which, as reported, had been gradually produced since the commencement of the cough; her bowels were confined and the pulse when unexcited, numbered 100.

On percussion, the upper regions of the thorax on both sides, emitted a marked dull sound, and the expansive powers of the chest were obviously diminished, especially on the left; the breathing was extremely feeble in the upper lobes of both lungs, and a strong resonance of voice or cough heard at the distal extremity of either clavicle, but more decided on the left.

She was ordered to take thirty drops of the following mixture every four hours :—

℞ Liq. Potassæ ℥i.
Tinct. Camphor C. ʒiii.

To obviate costiveness, from six to ten grains of the aloes and myrrh pill, were occasionally given. Her diet to consist of milk, eggs, fish and bread ; to avoid exposure, and keep the surface well clothed.

She continued this treatment until the middle of the following April, and was then dismissed entirely free from cough and other symptoms ; her pulse numbered about 75 in the minute ; she had gained flesh, and considered herself well. In the physical signs of the chest there had occurred no marked change.

In 1839 I had again an opportunity of seeing this young person ; she reports that her health has continued good. She had resumed her occupations as a house-maid, and still remained in her situation.

VIII. Early in 1839, I was called to see a young boy, named Henry Baldock, residing in St. John's Wood, who had been some months ill. He was a thin sallow-complexioned lad, aged eight years, born in India, of European parents, who had returned to this country nearly a year before. His health soon afterwards began to suffer. At the period named his condition was as follows :—

The appetite is irregular and capricious ; the abdomen tumid and often tympanitic : the motions slimy, variable in colour, and fœtid ; the bowels often tend to confinement. His pulse is extremely quick and irritable, generally beating 100 strokes in the minute, and the tongue coated towards its centre, but at the tip and edges red, with elevated papillæ.

Cough, and difficult respiration on slight exertion, formed marked symptoms of the case. The former was dry and hacking, occasionally only attended with slight viscid expectoration. An examination of the chest revealed an unfavourable condition of the lungs. On the right side, percussion gave a remarkably dull sound, in the clavicular and upper part of the mammary and scapular regions; resonance of voice and pulsation of the heart were distinctly heard at the distal end of clavicle, and the respiratory murmur was very feeble over the whole of the situations named. At the same points on the left side there were few signs of disease, the breathing in particular was puerile, and the percussed sounds clear. A strict adherence to the usual diet was directed; moderate alterative doses of hydrarg. c. creta with ipecacuanha were at first given each alternate night, and afterwards twice a week; his bowels were ordered to be opened gently with castor oil, when this was necessary, and he took continuously for about ten months thirty drops of liquor potassæ three times a day in simple water. In about two months the abdominal symptoms had so far declined as to induce an abandonment of the mercury, and those connected with the chest gradually subsided in about four: the alkali being afterwards continued more as a precautionary measure than any thing else. This boy still continues well, though he has lately passed through an attack of measles. His cough, of course, then returned, but abated as speedily as it usually does in favourable cases of the disease.

IX. A case, exceedingly analogous to the above, was brought under my attention about the same time, in the person of Miss M., a young lady aged twelve, the daughter of a gentleman high in the civil service of the East India Company, who had recently returned to this country. I need not enter largely on its details, seeing that the symptoms and physical signs were in their general aspect very nearly similar. Here, however, the left side of the chest appeared to show the greater tendency to disease. The treatment pursued was in principle the same, and followed by similar improvement, which to this time continues permanent.

X. Mrs. Wells, residing in Portman Mews, aged thirty-nine, married, was seen first in January, 1840.

She is a tall thin woman, of delicate aspect, who states herself to be the descendant of a family prone to chest complaints, and that in her own person she has long suffered from pectoral symptoms. She then was labouring under a severe attack of hæmoptyses, which twice recurred afterwards during the progress of her treatment. Besides this, her cough was unceasing, and her expectoration copious; hectic fever prevailed, and she could take no exercise without great dyspnœa being produced. Great dullness on percussing the upper regions of the chest, particularly on the left side; distinct resonance of voice and feeble respiration all concurred in illustrating the condition of the lung. The acetate of lead, with opium, mild laxatives, and blisters to the chest, checked the pulmonary hæmorrhage. She was then placed steadily on the alkaline plan, which, under the assiduous care of my friend, Dr. Roxburgh, was persevered in for many months with decided advantage.

Dr. R. informs me that during the progress of the case, Mrs. Wells wore on the chest a plaster, composed of one part Emplast. Cantharidis to six of the Empl. Resin.

I saw the patient a few days ago; she has had no return of her symptoms for many months; has gained flesh, and is, though constitutionally delicate, free from all marks of existing disease.

CASES

Tending to show the possibility of Tubercular Absorption.

I do not offer the three following cases, as at all demonstrating the fact that Tubercles were absorbed in any of them, in their crude state, but merely as presenting the best approximative evidence I have yet met with on the question: they will all at least be admitted to have presented very suspicious symptoms; and if we allow the original presence of tubercles in any of them, I cannot well account .to myself for the return of breathing, on other supposition.

I. W. Bull, aged twenty-six, a gentleman's coachman, was seen for the first time in the beginning of July, 1839. He is a full-faced but delicately complexioned man, with blue eyes, light hair, and reports himself of a family many of whose members have suffered from chest affections; has not himself been peculiarly liable to cough till about three months ago, when he was attacked with a severe cold and spitting of blood. The cough has continued ever since, and is now attended with a suspicious muco-purulent expectoration. The hæmoptysis, under an attack of which he now labours, occasionally recurs, but in more moderate quantity than at first. The least muscular exertion induces great dyspnœa; the pulse, when he is at rest, numbers about 90 beats in the minute. The bowels are moderately open, the tongue free from fur, but redder than natural, and presents elevated papillæ. Percussion practised over the right clavicular, mammary, and scapular regions, elicits a sound extremely dull, and the pulsations of the heart are distinctly heard under the distal end of the clavicle of this side. Vesicular respiration feeble over the whole upper portions of it, and in many points inaudible. Resonance of the voice, or cough, is also very clearly marked, when the stethoscope is applied under the acromial end of the clavicle. Percussion practised over the left side of the thorax in corresponding situations, elicits a sound but little different from that of health; the respiratory murmur is distinct, or even louder than natural, and resonance of the voice absent. From these circumstances I concluded that the upper lobes of the right lung were beset with tubercles, while the left was entirely or nearly free from them. Hæmoptysis being the most urgent symptom of the case, this man was first treated with acetate of lead and opium, taking eight grains of the former with two of the latter in divided doses during the day; repeated blisters were applied, and a moderate action on the bowels kept up by a solution of sulphate of magnesia in infusion of roses. He was desired to live on vegetable and farinaceous food, and avoid all active exertion. The hæmoptysis ceased in a few days, but recurred at intervals in small quantities for several weeks. His febrile symptoms abated, and his general condition was improved, but the cough and expectoration continued much the same, and his strength declined. On the 6th of August following, he began the following mixture:—

℞ Mist. camphor, ℥ij.
Infus. cascar. ℥iii ss.
Liq. potassæ, ʒii.
Tinct. hyociam. ʒi.

M. One ounce of this compound was taken every four hours, and after the first week the alkaline liquor was increased to three drachms. He continued this till the 25th of October, with a progressive decline in all the symptoms, taking no other medicine except a few grains of compound rhubarb pill, when the bowels were inactive. On that day he was dismissed, and soon after went into the country with his master, taking with him a supply of the alkali, which he promised to continue for some time, and to adopt a diet as much as possible composed of milk. His state at the period of his dismissal, as extracted from my note-book, was as follows:—" No recurrence of hæmoptysis since the beginning of August; cough entirely ceased; no pain of chest, but still short breathed on slight exertion. Pulse 80, bowels regular, appetite good, says he has gained strength and flesh." Early in April, 1840, I met this patient in the street accidentally, and procured the following statement:—He had remained in the country till a few weeks before, and continued his alkaline drops to the extent of half a drachm, three times a day, for two months. Though much exposed on the coach box, he had no return of the symptoms—his breathing was free, and the general functions properly performed. On the 6th of the following October, I again saw him, and had an opportunity of examining his chest; he then remained well. Over the upper regions of the right side, which had been at first chiefly implicated, the respiratory sound was naturally audible, except at one point under the distal end of the clavicle—there, it was quite wanting, and strong resonance of voice, if not pectoriloquy, existed over a circumscribed spot. On contrasting the results of this examination, with those previously obtained, we must be driven (assuming their correctness) to admit, that air had again found entrance to a portion of the lung from which it had been at one time excluded, for respiration was now much more audible than it had been when the treatment commenced. If the early consolidation really was produced by tubercles, then it follows, that those must have been absorbed in their crude state, the lung reverting to its original position; and though I am by no means disposed to contend strongly that such was the case; yet, coupling this one case with the others following, in which the same clearing of the lung occurred, I do not conceive that we are justified in denying the possibility of such an event. However this may be, whether the original dullness and absence of respiration depended solely on tubercles, or partly on temporary congestion of the lung, I think the case warrants the conclusion that these bodies did exist to a certain extent; and that a cessation of the symptoms followed the alkaline method of treatment which was used. Does the circumscribed spot under the right clavicle indicate the existence of a pulmonary cicatrix, or only tubercles in a latent state? I am unable to determine.

II. A. Vevers, aged 30, residing at No. 12, Gray street; of small stature and delicate aspect, I saw for the first time early in May, 1839, as a patient at the St. Mary-le-bone Dispensary. Her statement was, that from early life she had been peculiarly liable to chest attacks from slight causes; so much so, that for many years she had seldom been free from cough and dyspnœa on slight exertion. Her father had some years ago died of "decline," and she had lately lost a brother and sister, affected with the same disease. About a month before, she had suffered an aggravation of her symptoms, and is now afflicted with lancenating pains in the right chest, and expectoration occasionally tinged with blood; her pulse is quick and small, numbering from 100 to 110 strokes in the minute. She has been gradually losing flesh, and is now thin and weak; the bowels tend to costiveness, and her tongue is coated with a white mucous, except at the tip and edges which are red.

On examining this woman's chest for the first time, I made the following note:—

"Whole upper regions of right side of the chest, both anterior and posterior, emit a very dull sound on percussion; resonance of voice distinct under acromial end of clavicle; respiratory murmur feeble in upper lobes. On left side, slight resonance of voice; respiratory sounds rather feeble but distinct in upper lobes, very clear in lower. Percussion elicits a sound clear, compared with the right side, but less so than that found in a healthy lung." The diagnosis noted was as follows: "Tubercles in an unsoftened state, extensively located in upper lobes of right lung, and probably scattered through the left also."

Repeated blisters were applied over the right cavity of the chest, and her bowels occasionally acted on by mild aperients, with which small doses of mercury were combined. Her diet to consist, as much as possible, of milk and farinaceous food, eggs, white fish boiled, and animal broths. She took also in divided doses, during the day, a drachm and a half of the liquor potassæ in a vehicle of camphor mixture. This treatment was continued till the end of July, and on the 29th of that month, my note book supplies the following memorandum. "The breathing is considerably improved and much less hurried than before on muscular exertion ; cough trifling, and expectoration slight ; still complains of shooting pains in right chest ; pulse, after walking to my house, numbers 95." She was ordered to persevere in the same plan. The further details of the case I need not enter on ; suffice it to say, that she continued the alkali as originally prescribed until the end of the following September, and declined further treatment on the 27th of that month, considering herself then well. On that day I again minutely examined the chest, and made the following note :— "Over the scapular, clavicular, and mammary regions of the left side, the sound on percussion is clear, and the respiration distinct, but feeble. On the right side, at the same points, percussion gives a sound nearly equally natural, and vesicular breathing is all but normal ; but the resonance of voice still continues."

I leave this case in the hands of the reader with one only remark : that though it by no means proves the possibility of tubercular absorption, it tends so to do. I cannot conceive it probable that this woman's lungs were consolidated originally from any other cause than tubercle ; and yet we find a decided clearing of them between the period at which treatment began and that at which it ended ; and this without any marked expectoration.

III. In May, 1838, I was desired to see M. Stanley, a little girl aged 10, the child of a consumptive mother, and in her own person presenting all the characters of a strumous habit. Her symptoms were then chiefly abdominal ; the belly was large and tumid, the motions slimy, green, and fœtid ; the appetite irregular, and capricious ; the tongue coated white, with elevated red points ; considerable emaciation existed ; but though the pulse and respirations were inordinately accelerated on trifling exertion, she had little if any cough; in fact, was free from any decided pectoral symptoms. Tested by percussion, the upper regions of the left chest gave forth a sound less clear than natural, and decidedly less so than the same points on the right. On the left also, the respiratory murmur was extremely feeble, while on the right, as might be looked for at her age, it was distinctly puerile. The treatment then employed was exclusively directed to the abdomen. Mild mercurials, slight purgation with castor oil, and the persevering use of stimulating frictions, were the means employed. Under this treatment the child improved much, and was taken to the country for change of air in the following July. While there, she was attacked by measles, from which, as stated by her mother, she recovered speedily; but the cough, which had not been severe during her illness, remained troublesome, of a dry hacking character, and only occasionally accompanied by the expectoration of a transparent glairy mucous. She speedily began to lose flesh, and again became my patient, in August, under circumstances apparently more formidable than when we met some months before. Examination of the chest strongly corroborated this suspicion. Percussion on the upper regions of the left side of the chest brought out a sound extremely dull; respiration, except in a few points, was quite inaudible, and a strong resonance of the voice or cough was distinctly marked. The right side of the chest exhibited no signs of disease, it was naturally resonant on percussion, and strong peurile breathing existed throughout. Taking into account the whole circumstances of the case, I was induced to conclude that the consolidation of the left lung was produced by tubercles; that these had existed there to a limited extent when I first saw her, and that a large and sudden increase had taken place as a sequel to the attack of measles. She was ordered the following mixture:

℞ Infus. Cascarillæ ℥vss.
Liquoris Potassæ ʒiii.
Tinct. Hyociami ʒi.

M. One table spoonful to be taken every third hour.

Beyond this no medicine was prescribed except small doses of castor oil when required. Her diet to consist of milk and farinacious aliment, with either beef tea or white fish for dinner thrice a week. After continuing this treatment for a month her amendment was progressive and decided, she had gained flesh considerably, her cough had nearly left her, and she could run or ascend the stairs with only slight dyspnœa, or acceleration of pulse, which, when the body was at rest, numbered about 80 in the minute. On now examining the chest I was surprised to find that respiration had returned to a great extent in the upper lobes of the left lung; percussion gave a sound scarcely more dull than that of health, and no resonance of voice could be detected. The right lung still remained healthy. Her treatment was continued without variation until the end of October, when all her symptoms had disappeared. I have frequently seen this child since that date: the last time, a fortnight ago. She has now gone through two winters without a recurrence of her complaints, is free from cough, moderately stout, and to all appearance, free from disease of any kind.

CASES

Of Phthisis in its very advanced stage, benefited by the Alkali.

I. James Smith, aged forty-three, a smith, residing in Sumner street, Clerkenwell, became my patient at the Finsbury Dispensary in January, 1834, under circumstances which appeared extremely unpromising. He stated himself to have been afflicted with cough, great expectoration, occasionally tinged red, and difficulty of breathing on slight exercise for twelve-months before.

About two months previous his debility and emaciation had become so great as to prevent his working; he laboured under night perspirations, expectorated large quantities of thick creamy sputa, and, in fine, presented all the symptoms of a person in the late stage of phthisis. The physical signs fully confirmed what the general ones indicated, except that the extent of consolidation and evidence of excavations were not so great as from the symptoms might have been inferred. The left lung, tested either by percussion or the stethoscope, exhibited but slight signs of disease. On the right, percussion educed a sound extremely dull over and beneath the clavicle; in a few points the vesicular respiration was inaudible, or entirely masked by the bronchial sounds, and the voice strongly resonant under the outer end of the clavicle. About the centre of the mammary region, over a circular space of two inches in diameter, very distinct pectoriloquy was heard, and the gurgling sound of a cavity filled with softened matter, was frequently, though not at all times, audible.

All these circumstances led me to conclude that the patient was in a state far beyond the power of medicine to control, and I commenced his treatment without any hope of success, having in view nothing more than a palliation of symptoms, but still taking the chance which the alkali offers. The means I need not detail; repeated blistering, sponging the chest with dilute acetic acid, opiates or other anodynes, sometimes with, sometimes without the spirituous solution of ipecacuan, but always combined with the liquor potassæ, formed the general plan adopted. In about a month the improvement was so marked as to offer a faint gleam of hope, and the same treatment was continued with the change only of an increase of the alkali. For twelve months this patient was treated on the same principles, at the end of which period, having gained flesh and strength, with little cough and able to resume his work, he was discharged at his own request. I then lost sight of him for nearly a year, when I was again called on to attend him at home, in severe illness. He had been in bed some days, and then clearly suffered under the second stage of extensive inflammation of the lungs, of which he died on the succeeding day. An examination of the body was permitted, interesting on account of his former, not immediate disease. The cause of death was evident. An enormous effusion of soft unorganized lymph coated a large surface of the lungs; their substance presented in many parts the first stage of hepatization, and there was much water in the chest. At the point where pectoriloquy had been heard, the lung adhered strongly to the pleura—there was no cavity, but a dense puckered cicatrix dipped several inches into the lungs; a very few scattered unsoftened tubercles existed on both sides.

Query.—Was this a case in which treatment having arrested progressive deposition, permitted the powers of the system to extrude in a successful manner the comparatively small quantity of tubercular matter which originally occupied the lung? I believe it to have been so.

II. Mary Rhodes, aged forty-three, 17, Cumberland street, Middlesex Hospital. Has for many years been subject to cough and great dyspnœa on slight exertion. During the preceding winter she was seized with an unusually severe cold, which having continued, she applied to me in February, 1839. At first the cough was constantly attended by bloody expectoration, which has lately ceased. She now spits large quantities of a dense yellow phlegm, she perspires much at night, has become greatly emaciated, and is subject to occasional diarrhœa. The left side of the chest expands much less freely than the right, and each inspiration is there attended by an upward drag, distinctly visible. Percussion over the sub-clavicular, scapular, and axillary regions of left side, brings out only a very dead sound, and under the distal end of the clavicle very distinct pectoriloquy is present. On the same situations, on the right side, the respiratory murmur is generally very distinct; in some points, puerile, and the sound on percussion, normal. The pulse numbers 100 per minute. Her appetite is good, and she complains of pain in course of the large tracheal branches.

She was blistered on the chest repeatedly during her treatment. Powders, composed of 4 grs. Hydrarg. c. Creta, and 6 of Dover's Powder, were ordered every other night, and continued until the diarrhœa ceased. Anodynes, chiefly the extract of Conium, were afterwards continued in moderate doses each night. From the first she was placed on the alkaline liquor, in a bitter infusion, taking of the former 2 drachms in the day. Being a very poor woman I had no opportunity of regulating her diet, which was scanty and ill calculated for her situation. She was permitted a pint of porter in the day.

The patient continued her medicines until the 22nd of July, when she was dismissed. Her state was then as follows:—"Has gained flesh, says she feels well; coughs only a few times in the day; little or no expectoration, perspirations have ceased; pulse 85, rather intermitting; appetite good."

I saw this woman about six months afterwards, when the improvement still continued. Of her subsequent fate I can give no account; she appears to have left her then residence, and I cannot trace her. Whatever that fate may have since been, she appeared to derive great and unexpected benefit from treatment at the time.

III. In the month of February, 1835, I was called on to visit James Spittal, aged forty-three, residing in Whiskin street, Clerkenwell. The patient was, or had been, employed in some department of the watch trade, and though reduced in circumstances by long illness, still retained around him a moderate share of the comforts of life. When I first saw him, he had been confined chiefly to his bed for some weeks, presented an aspect of extreme emaciation, suffered from constant cough and copious expectoration. Hectic fever, night perspirations, and occasional diarrhœa prevailed; in short, he exhibited one and all of the symptoms which are usually supposed to indicate an aggravated case of phthisis in its most advanced stage.

These had commenced six months before, and progressed as usual, becoming more and more fully developed as time went on. He had years before been affected by cough and hæmoptysis, and was always considered a person of consumptive tendency.

It will not be supposed that, under the circumstances named, I could have expected much benefit from any treatment, beyond what might arise from means merely palliative; in truth, I expected none, and I believe it was at first more from habit than any thing else that I combined the alkali with anodynes and such other remedies as appeared best suited to fulfil symptomatic indications. Contrary, however, to those gloomy forebodings, a few weeks seemed to have wrought a very remarkable and unlooked for change. His breathing, cough, and fever were all relieved; his perspirations diminished, his appetite better, and he could in fine weather walk out with little inconvenience for a short distance.

These encouraging circumstances led me to direct my attention more particularly to the case, and especially to the condition of the lungs, whose physical exploration from the seemingly desperate nature of the symptoms had been before greatly overlooked. A recourse to percussion and auscultation did not tend to diminish alarm. On the left side the natural sounds of respiration were absent in the upper regions, bronchial breathing being strong; the chest externally was flattened, and fallen in beneath the clavicle, and the upward drag of the ribs distinctly marked on inspiration. Percussion gave a dull sound, and very distinct pectoriloquy was heard in the mammary region. On the right side the chest was relatively protuberant, and in some points the breathing sound remarkably strong, but harsh; the existence of cavities was here clearly announced.

The general plan of treatment he had before been placed on was continued, with an increase of the alkali, of which he took about three drachms during the day for many months.

During the summer of 1835 the patient improved rapidly, much to his own satisfaction, and not less to my astonishment, and towards its end was, for a weakly person, comparatively well. As winter advanced, however, he again exhibited his old symptoms in an aggravated form, and during the entire course of it, was confined to the house.

The alkali at this period was again resorted to as the basis of treatment, but coupled with anodynes, blisters, and such other means as symptoms indicated. In spring, he was again relatively well, and went through the summer of 1836 in comfort, and able partially to attend to his work.

In autumn he again declined, and during my temporary absence from town, died in November, as far as I could gather, from a sudden and severe attack of pneumonia excited by cold.

One circumstance occurred during the progress of this patient's illness, which appeared to confirm, in some measure, the advantage accruing from the use of alkalis. While progressing in a satisfactory manner, during the summer of 1835, he all at once became worse without any known cause. Merely by accident I found, at one of my visits, that he had, with a view of alleviating the cough, been eating considerable quantities of acidulated drops supplied him by a friend. On abandoning these, according to my direction, his symptoms became speedily improved.

In reporting this and the two preceding cases, I could desire most emphatically to disclaim any wish to have them considered as examples of what usually occurs when an alkaline treatment is persevered in; or of what indeed occurs under any treatment whatsoever; so far from this, these three cases are by far the most marked examples of benefit, I have seen from treatment, when this was commenced after the second, or reactive stage of phthisis had been fully established ;. but even these few, may if properly viewed, hold out to us a useful lesson, and direct our attention to a remedy, of whose efficacy when employed early, I cannot entertain any doubt whatever.

FINIS.

Plate I.

Fig 1.

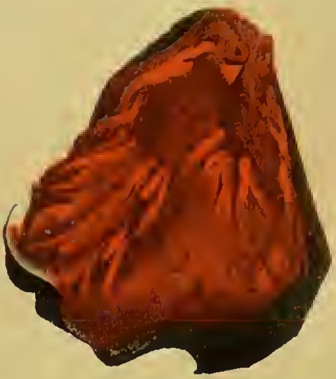


Fig 2.

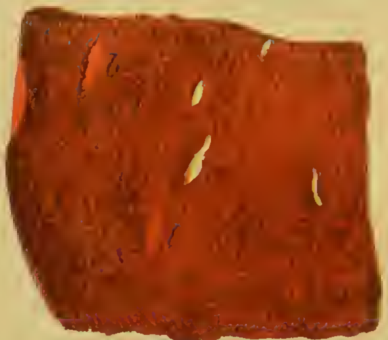


Fig. 3.

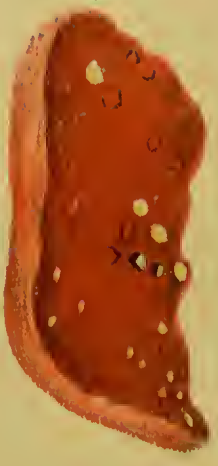


Fig. 4

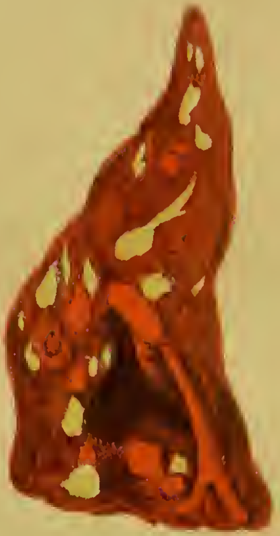


Fig. 5.

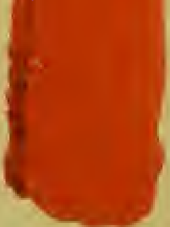
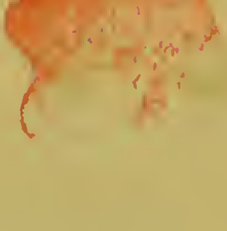


Fig 6.



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EXPLANATION OF THE PLATES.

PLATE I.

Fig. 1. Section made in the perpendicular direction of lobe on the lung of an infant aged one month, after injection of the entire organ through the pulmonary artery. The child was the offspring of healthy parents, and died apparently in consequence of neglect and improper nourishment; no appreciable lesion being discoverable on dissection.

The brighter red lines represent the larger arteries and veins indiscriminately, the colour of their contents being the same in all; the dull red shows the injected matter in its capillary distribution; and the black exterior boundary marks a portion of pluera covering the edge of the lobe, which assumes this hue in the act of drying.

Fig. 2. Perpendicular section of injected lung, taken from a child aged four years, who died from softening of the brain succeeding measles. The enlargement of the abdominal glands and those of the neck sufficiently attested the existence of a strumous constitution: the lungs, however, were entirely free from tubercular deposition. In this example the larger arterial branches traversed by the knife will be seen to contain red injection, while the appreciable veins are occupied by its colourless basis only—the red particles being retained in the minute vessels, whose convolutions form the larger portion of the plane represented by the plate.

Fig. 3. Horizontal section of lung taken from a child aged five years, the member of a highly strumous family, and presenting in its own person all the external signs of scrofulous disease. Its death arose from marasmus, consequent on obstruction of the mysenteric glands.

The lungs in this case contained, especially in the upper lobes of both sides, a considerable number of disseminated tubercles, not yet softened. The fact of separation of the constituents of the injection is here more broadly announced than by either of the preceding figures. The red cylinders mark the extremities of cut arteries; the white, those of intersected veins: the dark specks which in some instances are seen in connexion with vessels, indicate minute tubercles, which have blackened in the process of drying.

Fig. 4. Perpendicular section of lower lobe of lung in a young person, presenting all the characters of struma. The lungs were loaded with tubercles, some in a crude, others in a softened state. The black central portion indicates a small cavity, which had assumed the appearance on drying.

The figure is one which, contrasted with the others, shows the more decided separation of the constituents of injected matter in cases where the tubercular constitution is especially marked.

Fig. 5. Portion of injection as employed, taken afterwards from a large branch of the pulmonary artery, in the case which furnished Fig. 3.

Fig. 6. Portion of same injection, derived from the left auricle in the same subject, after having traversed the entire series of pulmonic vessels.

Note. All the figures in this plate are taken from specimens which had been dried after injection. The brightness of colour is, in some measure, modified by this mode of preparation, but the facts they are intended to illustrate remain unaffected.

For application of the phenomena indicated in plate, *vide* p. 150 to 157 of text.

Fig. 1

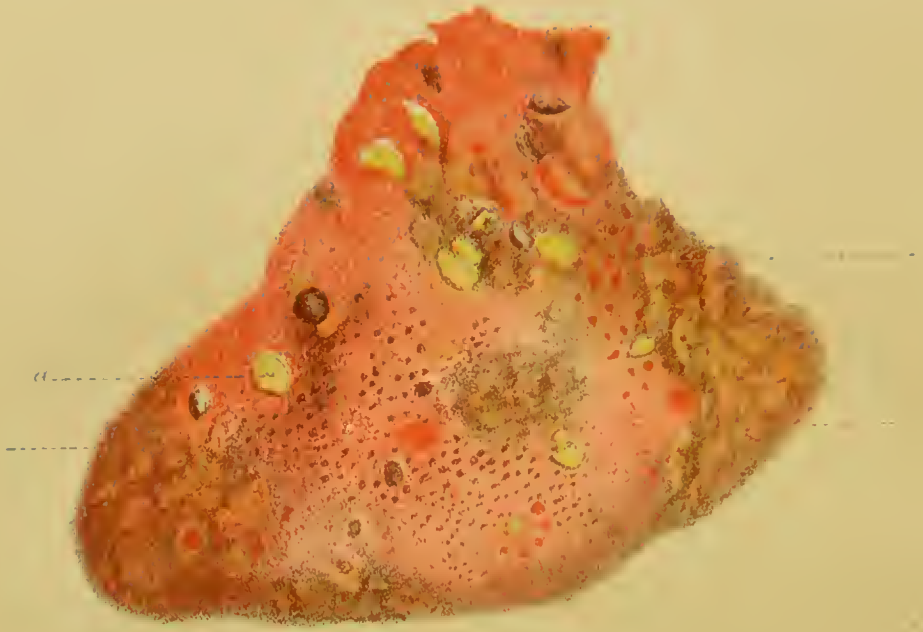

Fig. 2

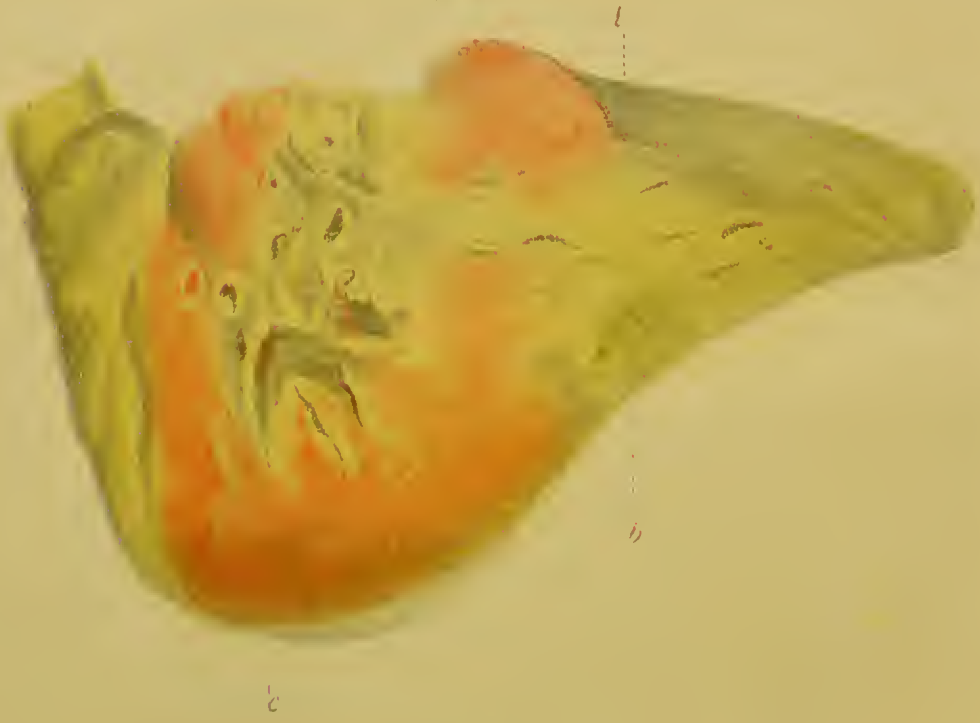

Fig. 3

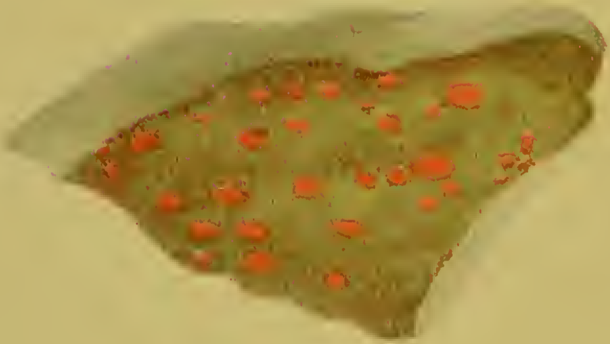

PLATE II.

FIG. 1. Transverse section of the upper lobe of right lung, after injection, in a young woman who died from phthisis in its advanced stage. The section was made a few lines below the most depending part of a large cavity, and the plane consequently represents the internal condition of such portions of a highly consumptive lung as are not yet excavated.

a. The intersected cylinder of a large pulmonary vein, carrying the discolourised injection; many similar to which, will be seen scattered over the surface of the figure.

b. The cut extremities of numerous bronchi of some size, apparently conveying air to more remote respirable parts, *vide* p. 189 in text.

c. Numerous branches of pulmonary artery, filled with the red injection as originally used. Larger branches of the same vessel appear also on various parts of the plate.

d. Portion of lung highly beset with crude tubercles, and impermeable by the air, in which a capillary circulation still seems to be carried on. The opposite edge of the figure, and a small central portion, exemplify a similar condition.

The deeper shades of red indicate such portions of the lung as are still respirable.

FIG. 2. Plane brought into view by the perpendicular section of one lobe, in the case of a child aged five years, who died from measles, having before indicated the existence of solid lung in various points. The lower end of the lobe was in this case permanently hepatized, doubtless from previous inflammation; but though the lungs were minutely examined, no tubercles could be detected.

a. Large bronchus, cut in a slanting direction.

b. Line of demarcation between respirable and consolidated portions, beyond which open vessels, derived from the right side of the heart, are not found to extend.

c. Respirable portion of lobe, minutely pervaded by injection.

d. Root of lobe, covered by pleura above the point at which the section begins.

FIG. 3. Section of lung taken from a person aged 50, who had long suffered from attacks of mild pneumonia, by which various isolated portions had become permanently consolidated. The lower portions of the

lobe were still in a great degree respirable, but their edges emphysematous.

The figure represents one of many solid portions, which intervened between the centre of circulation and distant parts of the lung, still comparatively healthy. The entire absence of colour in the ground of the figure sufficiently announces the absence of capillary circulation in the part traversed by larger vessels. Of those many are, without doubt, branches of the pulmonary *artery*, it is probable that some are *veins*, carrying back the injection unchanged. The distinction between the two classes I am unable to determine.

Note. The figures of this plate are copied from preparations which had been preserved in alcohol, which accounts, in some measure, for their superior brilliancy of colour as contrasted with those of Plate I. Independently of this, the matter of injection, or rather the basis of it, was different in the two cases; levigated vermillion was used in both examples; but in the injections, whose results are shown in No. 1, the vehicle of this was mutton tallow, a little reduced in consistence by the addition of about one-fifth of olive oil; while in those of No. 2 the injection used was that compound of wax, resin, and other ingredients employed commonly for anatomical purposes, under the name of " coarse injection," the composition of which is as follows :—

Yellow wax	16 oz.
Best resin..............	8 oz.
Turpentine varnish	6 oz.
Vermillion	3 oz.

This accounts for the superior whiteness of the matter which occupies the veins in the figures of Plate I., when contrasted with the dullish yellow tint of that contained in them, in those of Plate II.

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